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AN EXPERIMENTAL INVESTIGATION OF THE PRINCIPLE OF PROXIMITY IN THE VISUAL PERCEPTION OF THE RAT[1]

BY I. KRECHEVSKY

Swarthmore College

INTRODUCTION

The extent and even the existence of spontaneous grouping in sensory fields is a question of dispute between the *Gestalt* psychologists and the non-*gestalt* psychologists. For the *Gestalt* psychologist the existence of spontaneous groupings in sensory fields is of fundamental importance for all of their discussions of the perceptual process. Thus, for the *Gestalt* psychologist, the sensory units which make up perception are primarily determined by the specific field organization, to which the stimulus distribution gives rise. While not denying the influence, under certain conditions, of learning and experience, they do not conceive of the genesis of such perceptual units as being primarily due to learning. To use a very simple and rather common example, they point out that our perception of the dots in Fig. I is *perforce* a perception of a fairly well-defined grouping, rather than of an unorganized scatter of dots. Further, the factors making for this grouping are factors inherent in the sensory field itself—autochthonous factors of organization—and relatively independent of previous experience, reasoning, etc. The *Gestalt*

[1] The author wishes to record here his gratitude to Professor Wolfgang Köhler for his helpful criticisms in connection with the preparation of this manuscript.

psychologists are, of course, much more specific than this. They do not rest content with merely affirming the existence of some vague autochthonous principle of organization, but have discovered and defined several specific principles of

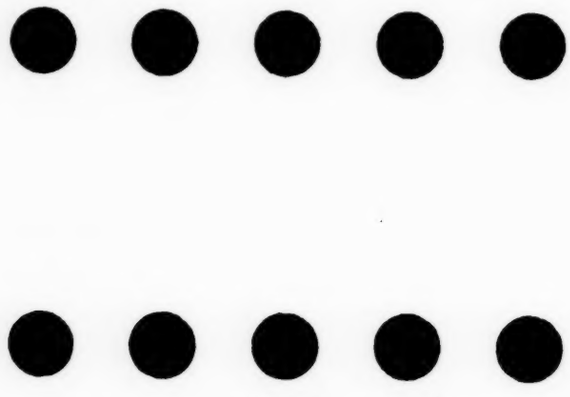

Fig. I

organization which enable them not only to say that our perceptual unit will be an *organized* one, but also to predict the *specific form* of the resulting organization. Thus, to return to our illustration, we could not only predict that we will see the dots of Fig. I as an organized grouping of dots, but that we will see them as organized in *two horizontal linear distributions*, and not, say, in five short vertical linear distributions, or a number of diagonal distributions, etc. (This is not an invarient response, since certain changes in the surrounding field *can* result in a *predictable* change in the perception, but the above prediction holds for most cases.) The specific principle of organization which the *gestalt* psychologist would appeal to in making this specific prediction concerning our perception is the principle of Proximity. This principle supposes first the general factor that "group formation is due to actual forces of attraction between the members of the group" (Koffka, 1935, p. 165) and secondly, the more specific factor that "When the field contains a number of equal parts, those among them which are in greater proximity will be organized into a higher unit" (*Ibid.*). This principle of proximity would thus enable us to predict the actual perception obtained.

The critics of *Gestalt* theory, in this instance, would not

deny that we *do* perceive the dots of Fig. I as two groups of horizontal distributions, but they would deny that there is anything 'necessary' about the perception, that the forces making for this perception are autochthonous. They would, instead, seek for an explanation in the past experience of the perceiving organism, in 'learning,' familiarity, etc. We are here, however, not interested in joining this debate, though we may paranthetically remark that in our opinion the *Gestaltists* have come off the victors on this question. What we were specifically interested in, in the planning of the present experiment were the following two questions: (1) Assuming the validity of the principle of autochthonous factors of organization in the sensory field, are these factors alone sufficient to account for the 'looks of things,' in all cases, or is some other set of factors necessary to make *effective* the operation of these principles? In other words, are the determining forces making for a particular organization to be found entirely within the sensory field, or are some of the essential determining forces making for a particular organization to be found outside of the specific sensory field in question and not derivable from the distribution of the field? (2) Assuming that the latter answer is the correct one for certain cases, then what is the nature of these necessary but non sensory-field forces, *i.e.*, what are the conditions which permit the functioning of the autochthonous principles of organization in these cases?

The general tendency of the *Gestalt* psychologist, in most of his experimental studies, has been to maximize the importance of the organizing forces within the sensory field and to minimize the importance of possible outside forces. (exceptions to this general tendency will be discussed later in this paper.) To explain the horizontal groupings of the dots in Fig. I the *Gestalt* psychologist would, in general, not feel it necessary to go outside of Fig. I. A complete analysis of the distribution of the dots in the figure would suffice for a prediction of the resulting perception.[2]

[2] Assuming, of course, a given organism with a given nervous system to do the perceiving. In this connection, in order to avoid any misunderstanding, we are *not* suggesting that the *Gestalt* psychologists believe that *gestalten* exist outside the organism,

Without at all denying the existence of autochthonous organizing forces within a sensory field, it is still possible to answer the same question with a slightly changed emphasis. Tolman, in discussing what he believes to be the difference between his 'sign-*gestalt*' and the more orthodox *gestalt*, points out that

> "According to a Simon-pure Gestalt-ist (. . . as we are conceiving such a one) these visual, kinesthetic and tactual stimuli will evoke a unique visual, tactual and kinesthetic configuration which will contain visual and tactual and kinesthetic qualities but will in addition present such properties as 'figure-on-groundness,' 'form,' 'solidity,' . . . etc. Finally, according to the Simon-pure Sign-Gestaltist . . . these same visual stimuli will be thought to evoke not just this configuration as a mere 'perceived' given, but rather some specific larger whole in which this merely pictured (*i.e.*, perceived) configuration will itself be embedded as one term in a larger means-end 'proposition' such, as that; 'that chair, if sat on, will lead to rest' (Tolman, 1933, pp. 394–95). And thus, argues Tolman, just as the *Gestaltists* have shown that the organization and form of the 'perceived' determines the very nature of the 'sensations' so is it the thesis of the Sign-Gestaltist that 'sign-*gestalts* determine Gestalten' (*Ibid.*, p. 410).

In simpler terms it seems to us what Tolman is saying is something like this: Just as we don't have 'pure sensations' divorced from organization and form, so don't we have 'pure organizations' in our perceptions divorced from certain immediately and concomitently experienced 'propositions' concerning these 'perceptions.' This would mean that the particular organization we would perceive would be a function of the autochthonous principles of organization as influenced by the needs, available means-and-readinesses, etc., of the animal. Thus, bringing Tolman's argument to bear on the specific question concerning us here, he would say that in order to describe and predict the specific perception resulting from the stimulation of Fig. I he would not only want to make

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that the forces of attraction, etc., operate 'out there' among the physical dots themselves, create segregated wholes, and then these 'wholes' project themselves into the nervous system of the organism. This is an erroneous reading of *Gestalt* psychology, but common enough an error to have caused Köhler to deny it specifically. "Organization in a sensory field" writes Köhler, "is something which originates as a characteristic achievement of the nervous system" (Köhler, 1929, p. 174); and again, "As a matter of fact the whole retina is a mosaic of indifferently related spots, and this is the case until sensory organization begins physiologically" (*Ibid.*, p. 177). Therefore, whenever we have occasion to use the phrase 'organizing forces within the sensory field' or 'forces of attraction between the members of the group' we mean, together with the *gestalt* psychologists, 'physiologically determined organizing forces within the experienced field.'

use of the principle of proximity but he would also want to know what the 'psychological situation' of the animal was at that moment. What were his needs, what did he know about the entire situation, etc. In other words, some of the forces determining the perception would derive from *outside* the particular sensory field.

Another way of describing these 'determining forces' deriving from without the sensory fields suggested itself to the present writer from some observations made in a previous experiment. In that experiment (Krechevsky, 1938*a*) we made a preliminary study of the operation of the principle of proximity in the perception of the rat. On the basis of the results obtained we concluded that the rat tends to group discontinuous stimuli according to the principle of proximity very much after the fashion of the relationship observed in the perceptual behavior of the human being. However, so strong did the 'forces of attraction' appear to be, that, after further consideration of our results, we were led to believe that some other factor than the principle of proximity must have been at work. In considering the possible factors (other than the organizational ones) which might be operating here, we made the following tentative analysis: The technique used in that experiment was to present the animal with what proved to be a difficult visual discrimination to solve. This discrimination could only be solved if the animal finally did achieve one specific visual organization rather than other possible organizations. In other words, the situation as a whole (the problem-situation) *required* that a given visual organization be made. Under those conditions of need the animal did finally achieve the organization in his perception. (Two visual patterns, one the positive and the other the negative, were used.) Phrasing the situation in terms of the *discrimination* to be made, immediately implies that the rat's perception of the positive stimulus field might not be entirely a function of the sensory field obtained from that card, but somehow a function of the complex "Positive-Card-'A'-to-be-discriminated-from-negative-card-'B'." In other words, we would thus be suggesting that forces deriving from *the need to make*

the A-B discrimination were in part responsible for the rat's perception, and these forces, of course, do not derive from the specific sensory field of card 'A.' This is merely an hypothesis, but one which could be tested. If we could devise an experiment where in one case the problem-situation did *not* force a particular organization, and in another case where the problem-situation *did* require that particular organization, but where, in both cases, the positive stimulus was the same, we could then test the influence of the differing problem-situations on the resulting perceptions of the identical positive stimuli.

We can now state our experimental question more specifically. Is the rat's perception of a given stimulus pattern of the type used in this experiment a function of the pattern alone, or is it also a function of the type of problem situation (discrimination) involved?

Apparatus and Procedure

The general experimental technique used was that of presenting two groups of animals with different visual discrimination problems, training them until they were able to make the necessary discrimination, and then running a series of test trials to obtain information on the nature of the rats' perceptions of the positive stimulus of each original pair. The apparatus used was the Lashley jumping-stand for visual discrimination learning.

Forty-two rats, divided into two main groups, were used as the subjects in the present experiment. These animals were all pigmented males, approximately three months of age at the beginning of the experiment, and were taken from the Swarthmore Psychological Laboratory stock. After an eight-day preliminary training period during which the animals were permitted to become accustomed to the apparatus and during which they were trained to jump through a white card as opposed to a black card, the two groups received the following training:

Group I (n = 19).—This group was given the 'difficult' discrimination. Immediately after the preliminary training period referred to above, these animals were started on the regular discrimination training. In order to control the possible effects of preference tendencies for certain visual patterns, the members of this group were divided into two sub-groups. Sub-group I*a* was presented with the discrimination set-up where Stimulus 'A' was the positive stimulus and 'B,' the negative stimulus (Fig. II, 'Training Series'). For the rats of Sub-group I*b*, the card 'B' was the positive stimulus, and card 'A,' the negative one. Sub-group I*a* consisted of eight rats, and I*b*, of eleven rats. The training schedule consisted of ten trials per day until each animal met the criterion set up as indicating mastery. To meet this criterion, the animal was required to make at least eighteen correct choices on any two consecutive days, *i.e.*, 18 correct out of twenty, or 90 percent correct.

The order of right-left presentations of the correct stimulus was varied according

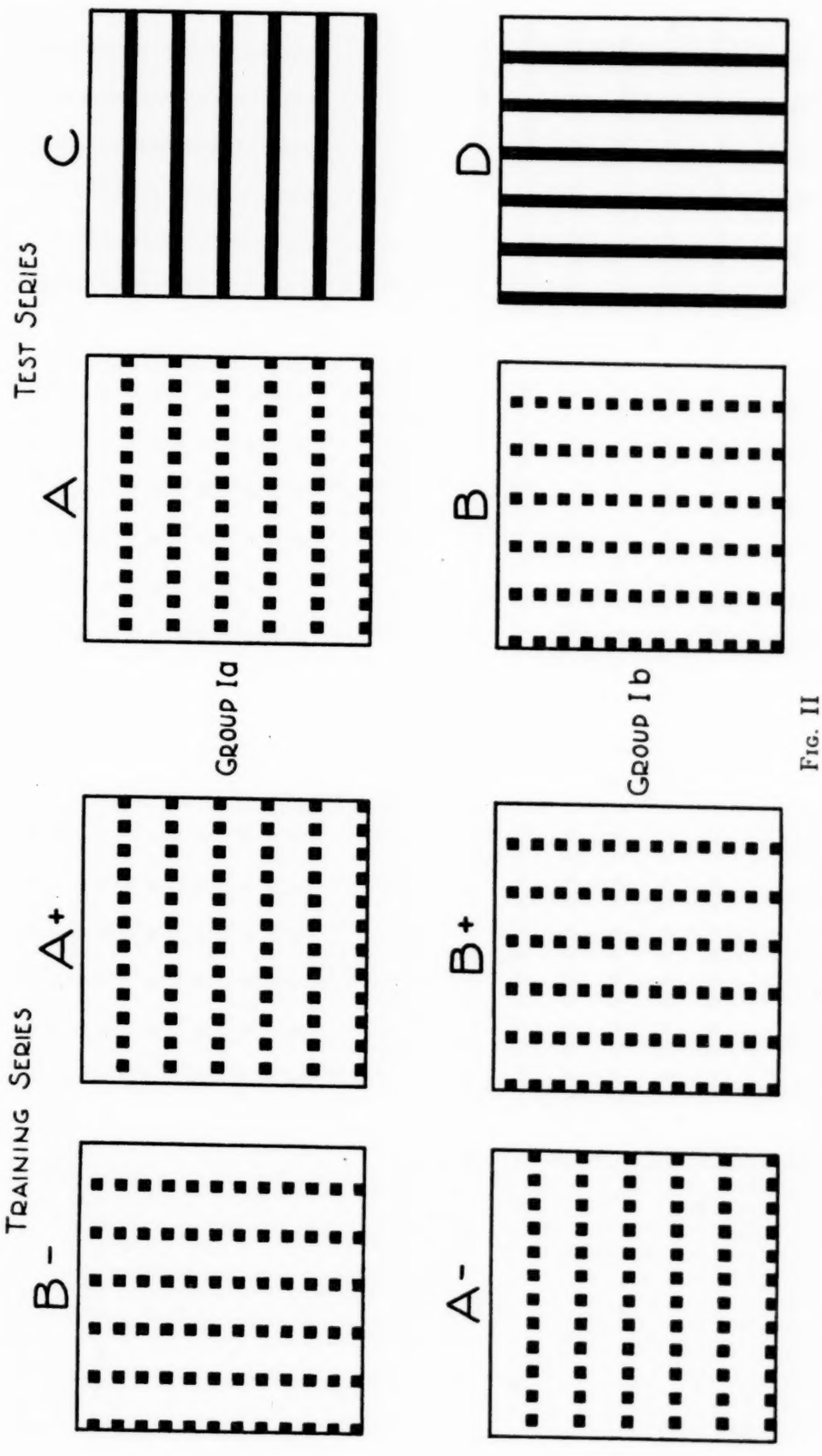

FIG. II

FIG. III

to a pre-determined schedule so set up as to attempt to avoid favoring spatial habits. Each stimulus presentation was allowed to stand until the animal had made the correct response, or until the animal had made four repetitive errors on any one trial. In the latter case the correct card was removed after the fifth successive wrong jump, and the animal was allowed to jump through the open window to food. Each incorrect response was recorded as an error, thus the maximum number of errors an animal was allowed to make on any one trial was five. This arbitrarily and artificially decreased the total number of errors for the animals, but we were not, in this instance, primarily interested in the learning scores of the animals, and felt justified in adopting any procedure which would speed up the original learning of the discrimination. The same procedure was used, of course, for all the animals in this experiment.

On the very next day after the animals of Group I had met the criterion of mastery described above, the individuals of Sub-group I*a* were given the test discrimination between cards 'A' and 'C' (Fig. II, 'Test'); while the rats of Sub-group I*b* were given the test discrimination between cards 'B' and 'D' (Fig. II, 'Test'). In these test situations a choice of either of the cards of the pair was 'correct' and was rewarded equally. These test trials, also consisting of ten per day, were continued until the animal again met a criterion of consistency, *i.e.*, until he had chosen the same card, or the same side, for at least 18 out of any twenty consecutive trials.

Group II (n = 23). This group was given the 'easy' discrimination. After the preliminary training these animals were started on their discrimination problems, and here also the animals were sub-divided. For Sub-group II*a* (eleven rats), stimulus-card 'A' was the positive stimulus and card 'E,' the negative: for Sub-group II*b* (twelve rats) stimulus-card 'B' was the positive stimulus, and card 'E,' the negative (Fig. III). For Group II, as for Group I, the animals were run on the training schedule until they had mastered their discriminations and were then given the test series. The rats of Sub-group II*a* were given the test-discrimination between cards 'A' and 'C,' while the animals of Sub-group II*b* were tested with cards 'B' and 'D.' Thus the animals of Group II were given the same tests given the animals of Group I.

The distribution of the discrete block squares on the stimuli cards used, and the test series, are such, we believe, as to provide for an adequate test of our experimental question. Considering first the cards used for the original training of Group I, we see that we have there two stimuli patterns of such a nature as to permit the operation of a definite autochthonous principle of organization—the principle of proximity (if such a principle is at all applicable to the perceptual process of the rat). It will be seen that any black square on card 'A' is nearer its neighbor to the right or to the left, than to its neighboring square above or below it. Just the reverse is true of the distribution of the squares on card 'B.'[3] In other words the principle of proximity would require that the squares on card 'A' be organized in a series of *horizontal* groupings, and card 'B,' in a series of *vertical* groupings. That this is what actually occurs in the perceptual process of the rat has already been shown (for cards with somewhat different dimensions) in a preliminary experiment (Krechevsky, 1938*a*). However, since the same kind of data will be available from this experiment, we shall reserve a more detailed discussion of this point until later in this report.

[3] The actual dimensions for the stimuli cards are as follows: For both cards the black squares were ¼ × ¼ inch. For card 'A' the horizontal distance between the squares was ¼ of an inch, the vertical distance, ¾ of an inch. For card 'B,' the reverse relationship held. For cards 'C' and 'D,' the solid lines were ¼ of an inch in width, and were separated from each other by ¾ of an inch.

Further, not only are cards 'A' and 'B' of such a nature as to *permit* the formation of horizontal and vertical groupings (*i.e.*, permitting the principle of proximity to function), but, most importantly for the purposes of our experimental question, the '*A*'–'*B*' *discrimination situation* is one which '*forces*' such a grouping. Since both cards 'A' and 'B' are equal in all characteristics such as brightness values, number of discrete black squares, etc., the animal, if he is to discriminate at all between the two cards, must make such a discrimination on the basis of the differences in the spatial distribution of the black squares.[4] We have here, to repeat, *a discrimination situation* which forces discrimination in terms of a specific kind of perceptual grouping of the discrete stimuli on cards 'A' and 'B.'

If we consider now the stimuli cards used in the discrimination training given Group II, we find an altogether different situation. The positive cards 'A' and 'B' are identical with the positive cards used for Group I, and so, assuming the operation of the principle of proximity, we might expect a horizontal-vertical grouping for these cards, *but,* the negative stimuli-cards in these latter cases (Card 'E') differs from the positive cards, in at least *three* respects. That is, the animals of Group II could learn the discrimination problem set them either on the basis of the relative brightnesses of the positive and negative cards (Card 'E' having more white surface than card 'A' or 'B'), on the basis of the total number of black squares on each card (Cards 'A' and 'B' having more black squares than card 'E'), or on the basis of the different organizations possible in the two cards (Cards 'A' and 'B' giving a horizontal and vertical grouping respectively, and card 'E,' a set of crossing diagonal groupings). Any one of these discriminations would be 'correct.' The important thing here is that the *discrimination situation* does *not* force any specific organization or grouping for the solution of the problem.[5]

This, then, is the important and crucial difference between the two groups. For Group I the principle of Proximity *must* work, if the problem is to be solved, and for Group II, the principle of proximity need not operate in order for the rats to solve the problem. The next step in the procedure, as has been indicated, is to test the resulting perception of cards 'A' and 'B' for the two groups.

Observation of the patterns on test cards 'C' and 'D' (the two cards opposed to the positive cards used in the original training series for both groups of animals) at once

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[4] The possibility of the animal's discriminating on the basis of extraneous cues peculiar to the specific cards used, was more or less eliminated by using different sets of cards throughout the training experiment. That is, we had alternate sets of 'A' cards and 'B' cards which were used.

[5] This does not necessarily mean that the animals of Group II could master their discrimination problem without making some sort of perceptual organization of the stimuli presented them. We could very well rephrase the differences between the two problem situations for the two groups by saying that for the animals of Group I to make the discrimination a vertical-horizontal grouping would have to be achieved, where for the animals of Group II to make their discrimination *some other visual grouping* (perhaps a more homogeneous one) would suffice. In other words, it isn't at all a question of visual organization being necessary for the rats of Group I and unnecessary for the rats of Group II, but simply one specific kind of organization being necessary for the animals of Group I and *another kind* of organization being adequate for Group II.

reveals that what we have here is a *completed* or more perfect form of the grouping which would result from the operation of the 'forces of attraction' between the scattered squares on cards 'A' and 'B' respectively, if the principle of proximity did apply to the perceptual process of the rat. That is, where we have the possibility of perceiving the squares on card 'A' as consisting of a series of horizontal discontinuous groups, on card 'C' we have an actual and easily perceived series of horizontal lines.

The three following sets of results might appear on the test trials. (1) The animal might prefer the original card ('A' or 'B') to the new card ('C' or 'D'); (2) The animal might show *no* preference between the two cards, taking 'A' or 'C,' for instance, indiscriminately; (3) The animal might prefer the 'more perfect' card to the original card, *i.e.*, taking card 'C' in preference to card 'A.' If any rat should make the first type of response, we could then only say that some sort of grouping did take place (for Group I) but the 'forces of attraction' were not of such a nature as to permit of a shifting from the original card to the 'more perfect one,' *i.e.*, the rat still perceived the black squares on Card 'A' as discrete black squares. In the case of the rats of Group II, we could say one of two things, either their perception of card 'A' was identical with the perception of the rats of Group I, *i.e.*, they did group the squares in a series of horizontal lines, or else, the rats of Group II never did group the squares in that manner and had learned the discrimination between their stimuli on some other basis (with some other groupings) and therefore on the test trials preferred the known positive card to a 'strange' and unfamiliar card.

If the rats should make the second type of response (*i.e.*, showing no preference) then we could more certainly say that the animals *had* organized the squares into horizontal and vertical groupings and were therefore reacting indiscriminately to either card, but that here, also, the 'forces of attraction' generated by the operation of the principle of proximity were not enough to cause a preference for the 'more perfect' horizontal groupings as opposed to the original stimuli cards.

Finally, if the animals gave the third type of response (*i.e.*, showed a preference for cards 'C' and 'D' as opposed to cards 'A' and 'B') we could say much more than that. We could, it seems to us, conclude that not only do the forces of attraction among the squares operate in accordance with the principle of proximity, but that these forces thus generated, are of such a nature and of such a strength as to make for a perception which results in the preference of the organism for a stimulus-complex where the discontinuous members in fact coalesce as opposed to a stimulus-complex where the members are still, in some degree, discontinuous.

To summarize: Three types of response are possible in the test situation, and each of these types result from different perceptual processes. A comparison, therefore, of the responses of the rats in the two groups should enable us to say something about the effect of different discrimination-situations on the perception of a single member of the pair to be discriminated between. Should the perception of card 'A' (or 'B') be similar for both groups, then we would be forced to conclude that the perceptual process was for the most part conditioned by the autochthonous organizational factor of proximity; on the other hand, should we find a difference in the perceptions of the two groups, we would be forced to conclude that the autochthonous factor of proximity, in this case, is insufficient to account for the perceptual process.

Data and Discussion

It will be remembered that each of the two groups was split into two sub-groups, in order to control the factor of a

possible horizontal or vertical preference on the part of the animals. In making the following comparisons between the two major groups, we shall combine the results of each sub-group within each larger group, hoping thus to equate, in each group, for any possible preference difference. It might be pointed out, however, that no very decided preference for either 'A' or 'B' was discovered to exist, and for most of the measures Sub-group I*a*, for example, differs very little from Sub-group I*b*. A more complete analysis of the data in this respect will be made later (see appendix A) when the various measures will be more meaningful. In the following discussion, therefore, when we refer to the behavior of Group I with card 'A,' we are really referring to the combined results of the behavior of the animals of Group I*a* and I*b* with card 'A' and card 'B.' The same procedure will be used for Group II.

Considering now the initial learning records of the two groups we see immediately that the two discrimination problems differed in some very obvious respects. In the first place the discrimination problem for Group I proved to be a more difficult one to master than did the discrimination problem for Group II. This is true, however, if we limit ourselves to the criteria of total trials and 'initial' errors. The criteria of 'repetitive' errors and total errors seem to tell a different story. Let us first confine our discussion to the trials and initial-error scores. Where the animals of Group I took 164.2 trials and made 60.04 initial errors before mastery, Group II required less than half that number of trials (67.8) and made a little more than half that number of initial errors (31.70) (Table I). Considering now the number of repetitive errors

TABLE I

LEARNING DATA FOR GROUPS I AND II

	Trials	Initial Errors	Repetitive Errors	Total Errors	Ave. Errors Trials 1–20
Group I	164.2	60.04	50.9	110.13	39.9
Group II	67.8	31.70	77.9	109.60	75.9
Difference	96.4	28.34	27.0	.53	−36.0
DIFF. / σ Difference	9.62	8.91	4.25	<1.0	8.91

made, we find that Group II made many more such errors than did Group I, with the result that when we add the repetitive errors to the initial errors for both groups, the difference in favor of Group I is cut down considerably, and no reliable difference seems to exist. This apparent internal inconsistency in the performance scores of the two groups is definitely explained and becomes significantly meaningful for our analysis when we consider the following facts. (1) It will be remembered that as part of the standard preliminary training on the Lashley jumping-stand the animals of both groups were trained to jump through a white card as opposed to a black card. This was done in order to familiarize the animals with jumping through cards. But this training actually teaches the animal more than how to jump through cards, it trains him to react *positively* to the *brighter* of two stimuli. Now, when the animals of Group I, after this preliminary discrimination training, are presented with the experimental cards (*i.e.*, when they start on their 'real' discrimination), this specific preliminary discrimination training can have very little influence on the distribution of their choices between cards 'A' and 'B,' since both these cards are equally bright. On the other hand, the situation is entirely different for the animals of Group II. In this latter case, as was pointed out above, the cards 'A' and 'E' differ from each other not only in the pattern or organization of their discontinuous stimuli, but also in brightness, *i.e.*, card 'E' is brighter than card 'A.' Thus, when the animals of Group II start in on their experimental discrimination, the distribution of their choices will be influenced by their preliminary discrimination training in such a manner as to make for a large number of incorrect responses, *i.e.*, they will, it can be expected, jump toward the brighter of the two cards (toward 'E') which in this case is the incorrect one. We thus have a situation where the animals of Group II would make many more errors than the animals of Group I at the very beginning of the regular training series. (2) In a previous experiment it was shown that for the Lashley jumping-stand, the first adjustment the animal has to make when attacking a discrimination problem,

is that of learning not to repeat a response when it proves to be incorrect (Krechevsky, 1938). That is, at the beginning of his training on a discrimination problem the rat shows a very strong tendency to repeat his immediately previous jump, whether it had proved to be correct or not. This repetitive tendency persists with a considerable degree of strength for at least two days (twenty trials) after which it seems to be given up. It was shown in the experiment just referred to that half of all the repetitive errors made during a sixteen day training period were made during the first two days. (3) It is obvious that a 'repetitive' error cannot be made until an 'initial' error is made, and therefore if in a given situation some factor exists which would make for a large number of initial errors during the first two days, a correspondingly large number of repetitive (and therefore total) errors would result—much larger than in a situation where no factor 'forcing' initial errors were present. These two situations are exactly what we have here with our two groups. The former situation exists for our Group II and the latter for our Group I. A closer consideration of our data bears out this analysis. Where the animals of Group I made but 31.5 percent of all their errors during the first two training days, the animals of Group II made 69.3 percent of all their errors during the same period. In terms of average errors, the rats of Group I made an average of 39.9 errors during the first two days and the rats of Group II made an average of 75.9—or almost twice as many. And yet, despite this initial handicap the rats of Group II learned the discrimination much more rapidly (see trial scores) than did the rats of Group I. All of this leads to the following description of the learning processes for the two groups. The animals of Group I started off their experimental training knowing very little about the discriminanda involved and found it relatively difficult to finally solve the problem presented to them. The rats of Group II, on the other hand, were presented with what proved to be a simple discrimination to solve, but at the very start were prejudiced in favor of the brighter (and wrong) card. They therefore had to first 'unlearn' this

discrimination, but once having learned to reverse the significates of the discriminanda, they proceeded very rapidly to master the discrimination. The very fact that the rats of Group II did respond to the brightness difference existing between cards 'A' and 'E' is of significance for us, since it immediately suggests that the differentia of brightness in this case was of behavioral importance for the rats and therefore they *could*, presumably, learn the 'A'–'E' discrimination on that basis alone (*i.e.*, on the basis of this simple organization) and never need achieve an horizontal-vertical organization of the two cards. This fact, of course, merely establishes the *possibility* of such a discrimination taking place, and we have yet to demonstrate that that is actually what occurred.

We are now ready for a consideration of the data obtained from the test trials.

It will be remembered that the animals were run in the test situation for ten trials per day until they had established some sort of well-defined preference, either for one of the two cards or for a response based on position, *i.e.*, to the right or to the left. In analysing the resulting records we have used four possible measures. The first measure of preference, and the one which in all test situations similar to the one used here is perhaps the most significant, is *the very first choice* made by the animal when presented with the test cards for the first time. The second measure we considered was based on the total number of choices made during the first day of the test trials, *i.e.*, the *first ten choices*. It so happened that some animals started off with one preference and about the second day, for one reason or another, shifted their preference, with the result that those animals which shifted their preference were continued until the second preference seemed to be established. This would mean that some animals were given more 'test trials' than others. Acutally it might be argued that after a number of trials, say ten trials, we no longer had a significant preference-test situation, since the animals would have learned, by that time, that a certain response was rewarded and would be choosing on that basis rather than on a 'pure preference' basis. This would mean that our mea-

sures which took into account the performances of the rats after the first day were not as reliable indicators of preference as the other measures. With this analysis we would tend to agree, and we would put more faith in the two measures described above than in our following two measures, but all possible measures will be considered in analyzing our data. The third measure used, then, was based on all the responses of the animal until he had established some one habit 'preference.' The fourth measure is given in terms of the habit itself which was finally established by the rat. In this measure we ask the question "Disregarding the actual distribution of choices, how did the animal *finally* end up, did he establish, as a final pattern of response, an 'A'-positive response, a 'C'-positive response, or a spatial response?"

TABLE II

TEST DATA FOR GROUPS I AND II *

	1st Choice	Trials 1–10	Total Trials	Final Preferences		
				Spat.	'A'	'C'
Group I	89.4	81.1(3.62)	74.6(2.09)	9	0	10
Group II	13.0	34.4(3.27)	38.7(2.00)	20	3	0
Difference	8.12	11.04	12.50			
DIFF.						
σ Difference	76.4	46.7	35.9			

* The data in the first three columns represent the percentage of 'C' choices made by the animals.

The data for all these measures are given in Table II, and the most significant measures are summarized in graphic form in Fig. IV. In the second column of Table II are given the total number of 'C' card choices made on the very first test trial for both groups (Measure 1). The difference between the groups for this measure is striking. Of the nineteen animals of Group I, seventeen chose the continuous lines (card 'C') and three, the discontinuous lines (card 'A'). Of the twenty-three animals in Group II, only three made the 'C' choice! In terms of percentages these data indicate that where 89.4 percent of the rats of Group I chose the 'C' card, only 13.0 percent of the animals of Group II did likewise.

When it is recalled that for both groups the 'A' card had been the positive card in the training series, the high score of Group I is strikingly significant. It may be argued, we believe, that this one measure is sufficient to establish a real difference between the two groups, since the first choice, in any preference test, is probably the 'purest' preference indicator.

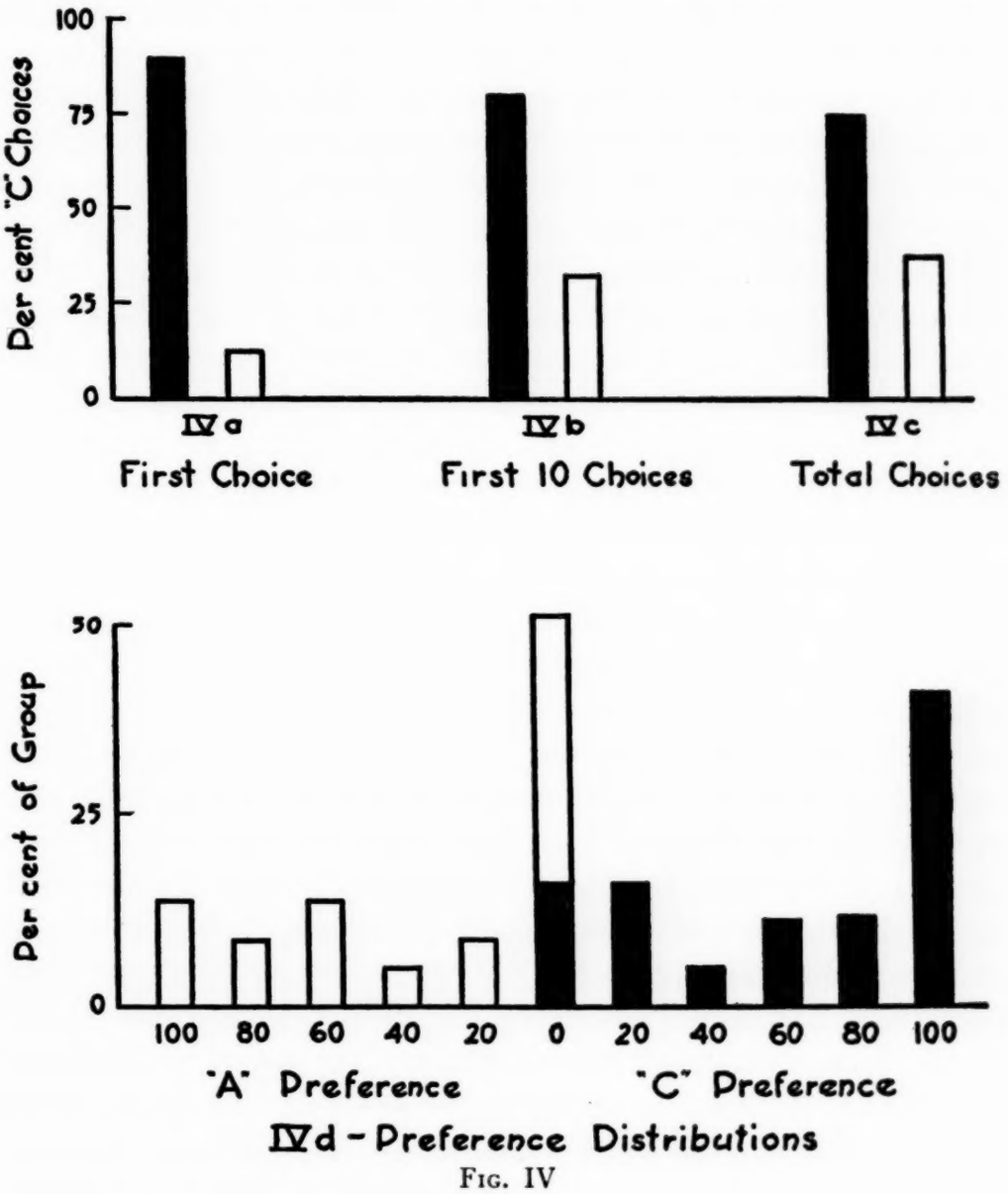

FIG. IV

In the third column of Table II are given the data for the second measure used. Here the total number of 'C' card choices made by the animals during the first ten trials are presented. Again the rats of Group I show a decided prefer-

ence for the 'C' card, where the animals of Group II show a preference for the 'A' card. For Group I, 81.1 percent of all the choices made were 'C' choices, whereas for Group II, only 34.4 percent were 'C' choices and the greater part of the choices (65.6 percent) were 'A' choices. The difference for 'C' choices between the two groups is thus 46.7 percent and that it is highly statistically reliable is indicated by a critical ratio of 11.04. In this connection we might ask another question. Granted that the difference between the two groups is a reliable one, is the preference of any one group reliably different from a zero preference? That is, are the scores of 81.1 percent for Group I and 34.4 percent for Group II, individually reliably different from a zero (or non-) preference score of 50 percent. Again referring to Table II we see that both these scores are reliably different from zero-preference (the figures in parentheses refer to the standard deviation of a 50 per cent expectancy), and we therefore feel justified in concluding that the animals of Group I showed a real (statistically reliable) preference for Card 'C,' whereas the animals of Group II showed a real preference for card 'A.'[6]. In the fourth column of Table II are given the data for the third measure used—the number of 'C' card choices for all the test trials. Again we find the same relationship between the two groups. Where 74.6 percent of all the choices made by the animals of Group I were choices of the 'C' card, only 38.7 percent of the Group II choices were to that card. This measure, of course, is not an independent measure, *i.e.*, it is highly correlated with our second measure, but is more inclusive. But here again the differences are reliable and each percentage taken by itself is reliably different from 50 percent. In the fifth column are presented the data for our final measure, the habit finally adopted by the animal. Again the same

[6] The following statistics were used in determining these reliabilities. The standard deviation of 50 percent for the total number of choices for Group I is 3.62 percent. That would mean that chance fluctuations from 50 percent would most probably not permit that percentage to go above 60.86 percent (*i.e.*, 50 percent plus three sigma of fifty percent). The actual obtained value was 81.1 percent, well above the limits indicated. Making a similar analysis for the choices of the animals of Group II, we find that the lower limit of a chance fluctuation from 50 percent would be 40.2, and the observed value was 34.4 percent.

direction of differences between the two groups is noted. Nine animals of Group I ended up with position preferences, and ten animals with 'C' card preferences.[7] For Group II, there were twenty position preferences established and three 'A' card preferences.

Up to this point we have been considering only the *average* performance of each group for measures two and three. The difference between the two groups is much more striking when we make an analysis of the *individual* behavior of the animals. Doing that we find that *not a single animal of Group I showed even a very slight preference of choices for the 'A' card and not a single animal of Group II showed even a slight preference for the 'C' card.* This is so whether we use the data from the second (1st ten trials) or the third (all the trials) measure. The distribution of the choices of the animals for the first ten trials are presented graphically in Fig. IV*d*. The scale on the abscissa ranges from 100 percent preference for the 'A' card (on the left of the scale) through zero preference (fifty-fifty choice) to a 100 percent preference for the 'C' card (on the right of the scale). The ordinate represents the percentage of animals in each group showing each indicated degree of preference. The black bars represent the scores of Group I and the white bars, the scores of Group II. Thus it can be seen that about 40 percent of the animals of Group I showed a hundred percent preference for the 'C' card, twelve percent showed an 80 percent preference, etc.; for the animals of Group II, about 14 percent showed a "hundred percent" preference for the 'A' card, ten percent showed an 80 percent preference for the 'A' card, etc., but no animal of Group I falls to the left of the preference scale (*i.e.*, the 'A'-preference side) and no animal of Group II falls to the right of the preference scale (*i.e.*, the 'C'-preference side).

[7] A number of these 'position' animals had started off with a 'C' preference, but during one trial or another would make a mis-step and fail to get through the 'C' card. They would then try the other side and from then on, persist in a spatial response to that side. It must be remembered that a spatial habit is much easier for the rat to establish than a habit based on a visual discrimination. The same thing occurred, of course, for the animals of Group II.

On the basis then of all the measures used it appears that the animals of Group I, when presented on the test trials with a choice between the original card 'A' and a 'new' card 'C,' show a strong tendency to prefer the 'new' card 'C,' though some of the animals fail to show any preference whatsoever between the two cards. On the other hand it appears that when the same test is given to the animals of Group II, there is a strong tendency for the rats to prefer the original 'A' card while some of the rats show no preference between the two cards.

On the basis of our analysis of the three possible types of response in this test-situation (page 507) we would make the following interpretations concerning the perceptual processes of the two groups of rats resulting from stimulation by the 'A' card in the original training series. (1) *All* the animals of Group I organize the discontinuous visual stimuli on card 'A' according to the principle of proximity, very much after the fashion of the relationship observed in the perceptual behavior of the human being. (This interpretation is based on the fact that all the animals of this group displayed either the type 2 or type 3 response in the test trials.) (2) The perceptual process for *most* of the rats of Group I involve 'forces of attraction' between the discrete members of the visual group on card 'A' of such a nature as to cause the animals to prefer the continuous *gestalt* over that of the discontinuous *gestalt* even though the original training be on the discontinuous stimulus-complex. (This interpretation is based on the fact that most of the animals displayed the type 3 response in the test trials.) (3) *Some* of the rats of Group II also organize the discontinuous stimuli on card 'A' according to the principle of proximity. (This interpretation is also based on the fact that some of the Group II rats showed the Type 2 response in the test.) (4) Some of the rats *may* not have acquired this particular organization at all. (This interpretation is based on the fact that some of the rats gave the Type 1 response in the test.) (5) The perceptual processes of *none* of the rats of this group involved 'forces of attraction' of such a nature as to cause, in the test trials, a preference for the continuous *gestalt*.

It seems fairly conclusive, then, that the perceptual responses of the rats to the stimulus-complex 'A' differ for the two groups used in this experiment.

Returning now to the original questions asked in the INTRODUCTION we feel justified in making the following tentative answers.

1. Inasmuch as the positive stimulus cards for both groups were identical and yet the resulting perceptual response differed, we must answer the first question posed by saying that the determining forces making for a particular sensory field are *not* to be found entirely within the sensory field, but instead it appears that some of the determining essential field-forces, in this case, derive from outside the given sensory field. This does not deny the existence of autochthonous organizing forces (actually the data of this experiment and the reasoning involved in our analysis *require* the existence of such factors) but it does deny their adequacy for explaining the 'looks of things' in this situation. Something else is required.

2. Since the only difference between our two groups was that of the difference between the two discrimination-situations and the resulting types of discrimination necessary, *i.e.*, in one case the situation was such as to make absolutely necessary a *particular* type of perceptual grouping and in the other case this was unnecessary, we might therefore say that only when the situation is such as to create a 'need' for a particular type of perceptual organization (as against other, and more simple, possible organizations) will that organization take place. And this 'need' derives, of course, from factors outside the sensory field—at least in part.

The word 'need' as used here is difficult to define clearly. That is a fault, perhaps, of the fact that the further we get away from the well-known and obvious 'needs,' such as the need for food, for sex, etc., the less clear-cut are our physiological correlates and the less clear-cut the behavior tests to be applied. We mean by this 'need' something like Tolman's 'subordinate goal.' That is, we are conceiving of a system of needs, an hierarchy of 'needs.' Let us assume that the

first and fundamental need of the hungry rat in our problem situation is the need of food. The obtaining of food, if he is able to do so relatively immediately, he will do without making any other adjustments to the situation. The primary need of food can be satisfied without first satisfying other needs, *i.e.*, the need to be able to discriminate between lengths of paths, position of correct paths, significates of certain visual stimuli, etc. If, however, he finds a barrier between himself and the food, other needs and other goals then and there become generated or begin to function. First there will be the need of making certain propositions about the consequences of certain responses to certain discriminanda, *i.e.*, he will begin to make 'hypotheses,' and just as the need for food immediately and intrinsically conditions the psychological forces effecting the animal, so the 'need' for determining the significates of certain visual discriminanda (in our situation) will immediately and intrinsically condition the so-called sensory field forces. Obviously the forces which will be generated by such a need (on the physiological level) are not the only forces which will condition the organization of the sensory field, there are other forces working at the same time and in an interdependent fashion—the so-called autochthonous forces of organization (also on the physiological level). Carrying our suggested analysis further we would then say that once this 'need' for discovering the significate of the discriminanda has been satisfied, one set of forces responsible for sensory organization no longer functions and further reorganizations of the field do not occur very readily. We would also have to assume that there exists an hierarchy of organizations, from the simpler, the more readily achieved to the more difficult. Thus, in our experiment, for Group II, one of the early, or more simple or more homogeneous organizations may have been that of 'many squares' versus 'few squares,' and it enabled the animal to satisfy the need of discovering the significates of the discriminanda involved and therefore the perception of card 'A' was a perception which might be described as an homogeneous organization or as 'a many squared card.' In the case of

Group I a more articulated organization was necessary, since to perceive card 'A' as a 'many squared card' did not permit it to be differentiated from card 'B' and therefore did not satisfy the need involved. In this latter case, therefore, the need persisted until a 'working' perception did occur.

It seems to us that what we have been suggesting above, in a very loose manner perhaps, is the same sort of thing Tolman has been doing with his 'sign-*gestalt*.' The particular merit of Tolman's concept of 'sign-*gestalt*' lies in his continued and experimental insistence upon the interdependence of the forces we too often segregate under the two categories of 'motivation forces' (the forces Kurt Lewin works with, as well as Tolman) and 'perceptual organizing forces' (the forces which are the primary concern of the work of Köhler, Koffka, and Wertheimer). In very many cases both sets of forces operate simultaneously and on the same level. This does not mean, of course, that there are no cases where one set of forces is not the more significant or the more important set, but it does suggest that more experimental attention needs to be paid to the very many situations where both sets of forces are of equal and immediate importance.

At this point it may be of some value to compare the position we have just stated with the position of some *gestalt* psychologists.

Let us first state that it appears to us that there is no real or important difference involved, but that there may be a slight difference in emphasis. We pointed out in the INTRODUCTION that the general tendency of the *Gestalt* psychologist has been to maximize the importance of the organizing forces within the sensory field and to minimize the importance of possible outside forces. This kind of emphasis is, we believe, not necessarily due to a *theoretical* bias in favor of the importance of one kind of force as opposed to the other, but due, rather, to the choice of the *experimental problems*. That is, the type of problem the *Gestalt* psychologist has concerned himself with (excepting the work of Kurt Lewin and the work of Köhler with Apes) was one in which the 'organizing forces' were relatively more important than

the 'motivational forces.' When, however, Köhler deals with psychological situations where 'motivational forces' could play a part, the interdependence of these two types of forces is very clearly recognized.[8] But, what we have been hoping to do with the analysis of the present experiment, is not merely to recognize that such forces must be dealt with in an interdependent fashion, but rather to experimentally define the specific interactions for at least one specific case. We have used a well-defined 'autochthonous principle of organization' and a fairly well-defined 'need situation' (a simple discrimination problem) and have investigated the inter-relations of the resulting forces. In other words we are suggesting the need for more experimental work which combine the 'motivational analyses' and the 'organizational analyses' with the hope that we shall then be able to arrive at certain well-defined and specific laws about the inter-relations of the two types of forces under consideration here—laws as specific as the laws concerning the forces of organization or the 'need forces' of Lewin and Tolman.

Summary and Conclusions

To test the assumption that under certain conditions the perceptual process of the rat is conditioned by forces of motivational origin as well as by certain 'autochthonous forces of organization,' an experimental situation was so set up as to present two groups of animals with two different discrimination situations, differing in difficulty of discrimination but having identical positive stimuli. In one case the positive stimulus could be differentiated from the negative stimulus only when one particular visual organization could be achieved by the subject; in the other case, the positive stimulus could

[8] In discussing some of his experiments with the Apes, Köhler writes, "After a short while we see the chimpanzee looking around for a stick. Evidently this attitude is not less determined in the total situation than was the direct tendency toward food. But again this new attitude has remarkable consequences upon the objects of the field. . . . The tree with its branches . . . may for a long time remain 'one thing,' too much a unit optically to let the functional value of a stick enter the branches, since these are not seen as optically real parts—at least by the chimpanzee. If finally this unit is destroyed under the pressure of the subjective attitude of 'seeking a stick,' we certainly have a case of a unit changed by the subjective attitude," (Köhler, 1925, p. 723).

be discriminated from the negative one without this particular organization taking place. Upon testing for the resulting perception of the positive stimulus it was discovered that the rats trained in the former situation did achieve the organization to be expected from the operation of the principle of proximity (used as the autochthonous organizational process in this experiment), whereas in the latter case a different (and perhaps more simple) organization had occurred. On the basis of the analysis thus made, the following conclusions are suggested:

1. Under certain conditions of need, such conditions being induced by the fundamental needs of the organism and conditioned by the specific problem situation and significate-attributes of the discriminanda, the rat tends to group discontinuous visual stimuli according to the principle of proximity.

2. The perceptual process, under those conditions of need, involves the operation of 'forces of attraction' between the members of a visual group of such a nature as to make the rat prefer the continuous *gestalt* over that of the discontinuous grouping even though the original training be on the discontinuous stimulus-complex.

3. When these conditions of need are not present, or present with lesser force, the data seem to indicate that the organization to be expected on the basis of the operation of the principle of proximity may not take place, under the conditions of our experiment.

4. The perceptual process which does occur, under the conditions of absence of specific need, is not of such a nature as to involve 'forces of attraction' between the members of a visual group which would cause the animal to prefer the continuous *gestalt* over that of the discontinuous one. Instead, it is suggested that the perception achieved may involve a less articulated, more homogeneous, organization.

Appendix I

The data for the comparisons between the sub-groups, I*a* vs. I*b*, and II*a* vs. II*b*, are presented in Tables III through

TABLE III

LEARNING DATA FOR GROUPS I*a* AND I*b*

	Trials	Initial Errors	Repetitive Errors	Total Errors
Group I*a*	167.5	62.1	54.2	116.3
Group I*b*	161.8	59.2	48.6	107.8
Difference	5.7	2.9	5.6	8.5

TABLE IV

TEST DATA FOR GROUPS I*a* AND I*b*

	1st Choice	Trials 1–10	Total Trials	Final Preferences		
				Spat.	'A'	'C'
Group I*a*	87.5	76.3	65.5	5	0	3
Group I*b*	90.9	84.5	84.6	4	0	7
Difference	3.4	8.2	19.1			

TABLE V

LEARNING DATA FOR GROUPS II*a* AND II*b*

	Trials	Initial Errors	Repetitive Errors	Total Errors
Group II*a*	68.2	33.1	81.4	114.5
Group II*b*	67.5	30.3	74.8	105.1
Difference	.7	2.8	6.6	9.4

TABLE VI

TEST DATA FOR GROUPS II*a* AND II*b*

	1st Choice	Trials 1–10	Total Trials	Final Preferences		
				Spat.	'A'	'C'
Group II*a*	9.0	34.6	35.5	9	2	0
Group II*b*	16.6	34.2	34.2	11	1	0
Difference	7.6	.2	1.3			

VI. A consideration of the data there presented immediately shows the equivalence of the sub-groups for equivalent conditions, in all respects, but one. That is, it apparently mattered very little whether the correct stimulus were card 'A' or card 'B.' The learning time (in terms of trials or errors) and the test results were equivalent. The only statistically

reliable difference is that obtaining between Sub-group I*a* and I*b* with respect to the percentage of 'C' choices over the entire testing period. Here the difference of 19.1 percent has a critical ratio of 4.88—obviously a reliable difference. However, it should be noted that *both* these sub-groups showed a real preference for the 'C' card. That is, the difference is not one of direction between the two groups, but one of degree of preference. Just what this means, if anything significant, we are not prepared to say, in the light of the fact that the other measures of preference for these two sub-groups do not differ at all. Obviously, what happened was that the animals of Sub-group I*a* started off with a decided preference for the 'C' card, but then, for one reason or another, switched to a position preference. This may be a result of some uncontrolled condition existing during the training period for this group (each sub-group was run at different times).

In general, however, we find no significant difference between the two sub-groups within each group, and this fact not only justifies our combining the two sub-groups for the major comparisons made in this report, but adds to the reliability of the differences between the two major groups, since each sub-group can be considered a repetition of the experiment, with different rats, at different times, but with the same results.

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References

KOFFKA, K., *Principles of gestalt psychology*, New York: Harcourt Brace & Co., 1935.

KÖHLER, W., An aspect of *gestalt* psychology, *J. Genetic Psychol.*, 1925, **32**, p. 691.

KÖHLER, W., *Gestalt psychology*, New York: Horace Liveright, 1929.

KRECHEVSKY, I., A study of the continuity of the problem-solving process, *Psychol. Rev.*, 1938, **45**, p. 107.

KRECHEVSKY, I., A note on the perception of linear *gestalten* in the rat, *J. Genet. Psychol.*, 1938, **52**, p. 241

TOLMAN, E. C., Gestalt and sign-gestalt, *Psychol. Rev.*, 1933, **40**, p. 391.

CONDITIONING OF THE ELECTRICAL RESPONSE OF THE CORTEX

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For the purposes of this study we are going to assume that when the alpha rhythm is obliterated or strikingly depressed in relation to the onset and duration of a stimulus, a response has occurred. Therefore, if such a response appears with respect to an originally ineffective stimulus by virtue of its association with another and effective stimulus, conditioning of the electrical response of the cortex has been demonstrated. To test this possibility it was necessary to select two stimuli which had markedly different effects upon the brain wave patterning. Auditory and visual stimuli fulfilled the requirement.

In two previous studies conditioning of the electrical response of the cortex has been mentioned. Loomis, Harvey and Hobart (**6**) have reported that following a previous experience of tone and light, ". . . conditioning is marked in some persons and not in others." Cruikshank (**3**) found that it was necessary to vary the interval between an auditory preparatory signal and the exposure of the light in order to avoid conditioning. Neither study gave any quantitative data to establish the fact of conditioning electrical potentials.

Experimental Procedure

A pure tone of 180~ and approximately 45 db above threshold was used for the conditioned stimulus. A light of approximately 0.0087 ft candles from a neon bulb was used for the unconditioned stimulus. The relative effectiveness of the tone and light as stimuli was determined for each subject prior to paired stimulations.

For the 8 subjects tone was effective in 11 percent of 643, and light in 99 percent of 344, readable records.[1] These results validate our choice of stimuli.

[1] For tone 12 percent and for light 34 percent of the records were unreadable because the waves were reduced or disappeared too gradually following stimulation, the alpha rhythm was absent at stimulation, or the waves disappeared simultaneously with the appearance of the stimulus.

The temporal interval between the beginning of the two stimuli when presented together ranged from 0.48 to 0.75 sec. with a mode of 0.55 sec. This interval was chosen in order that any alteration in the brain wave patterning due to tone could be revealed before the appearance of the light. The duration of the tone ranged from 0.73 sec. to 1.00 sec. with a mode of 0.80 sec. The light appeared during the last 0.25 sec. of each double stimulation. The duration of the light never varied.[2]

The frequency of stimulation was determined by the condition of the brain potentials, in that an attempt was made to present the stimuli only when a strong alpha rhythm was present. In a great majority of the records this attempt was successful. It was necessary to stimulate under these conditions in order to study the effect of the stimuli upon the waves.

The following routine was observed with each subject: determination of the effectiveness of tone alone; determination of the effectiveness of light alone; paired stimulations; and determination of the effectiveness of tone alone after paired presentations. The last two conditions were repeated several times. No interruption occurred throughout the entire routine which generally consumed one hour.

The subject sat with his eyes open in a completely darkened and semi-soundproofed room. Without mentioning the explicit purpose of the study, the experimental routine was explained to him. He was asked to relax and to remain as passive as possible. Such conditions as muscular tension and drowsiness could be detected by the nature of the brain waves appearing.

Six men and two women served in the experiment. Three of these subjects were staff members and four were advanced graduate students, all of the department of psychology. The other subject was a graduate major in another department. The possession of a dominant alpha rhythm was the main criterion for the choice of subjects.

The brain potentials were recorded from the occiput with an inactive electrode on the lobe of one ear.

Results

The records were evaluated on the basis of three measures; effectiveness, latency, and perseveration time. By effectiveness is meant an obliteration or a striking depression of the alpha rhythm related to the onset and duration of the stimulus. Latency is the temporal interval between the instant of appearance of the stimulus and the obliteration or striking depression of the brain waves. Perseveration time is the temporal interval between the cessation of the stimulus and the reappearance of the alpha rhythm.

Increased effectiveness of Tone when Paired with Light.—In order for a record to be read as effective to tone in the paired stimulation series the obliteration or depression of the alpha rhythm had to occur either before the light came on, simul-

[2] A pendulum was used to determine the duration of the tone. Because it was not always released simultaneously with the onset of the tone, variation in the duration of the tone and consequently in the interval between the beginning of the two stimuli resulted.

taneously with the appearance of the light, or at such a point after the light had come on which would give a latency to light shorter than the shortest latency read to light alone for the subject under consideration. The great majority of records read to tone fell into the first class (Fig. 1).

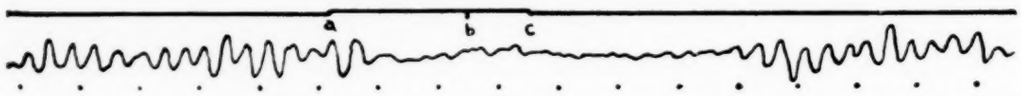

FIG. 1. Record showing the electrical response of the cortex to tone in the paired stimulation series. The latency of the response was read as 0.16 sec. The interval *ac* = duration of tone; the interval *bc* = duration of light. The perseveration time was read as 0.88 sec. Time marked off at bottom of record in 0.25 sec. intervals.

The statistically significant increase in effectiveness of tone during the conditioning series as compared to that of tone alone, pre-conditioning, is the essential finding of this study (Table I). This table shows that during the paired

TABLE I

RELATIVE EFFECTIVENESS OF A TONE PRESENTED ALONE (PRE-CONDITIONING) AND WITH A LIGHT IN DEPRESSING THE ALPHA RHYTHM

	Individual Subjects																Subjects Grouped	
	I		II		III		IV		V		VI		VII		VIII			
	% Eff.	N	% Eff.	N	% Eff.	N	% Eff.	N	% Eff.	N	% Eff.	N	% Eff.	N	% Eff.	N	% Eff.	N
Tone alone (pre-conditioning).	8	112	13	98	6	55	13	87	10	83	12	60	6	36	14	112	11	643
Tone with light.	42	154	40	162	26	171	22	144	34	243	35	225	38	110	41	176	35	1385*
Diff.....	34		27		20		9		24		23		32		27		24	
CR.....	7.7		5.4		5.9		1.3		5.3		4.4		5.2		5.4		12.0	

* A total of 1749 records were obtained. Of this number 21 percent were unreadable for the reasons previously stated.

stimulations the electrical response of the cortex preceded the light (unconditioned stimulus) in 35 percent of the records (all subjects). Only one subject failed to show a statistically significant increase in effectiveness of tone with light. It may be noteworthy that this subject was the graduate major in another department.

Figure 2 shows graphically not only the increased effectiveness of tone when it is paired with light, but also a con-

tinued high level of effectiveness with repeated presentations that is not characteristic of tone alone.

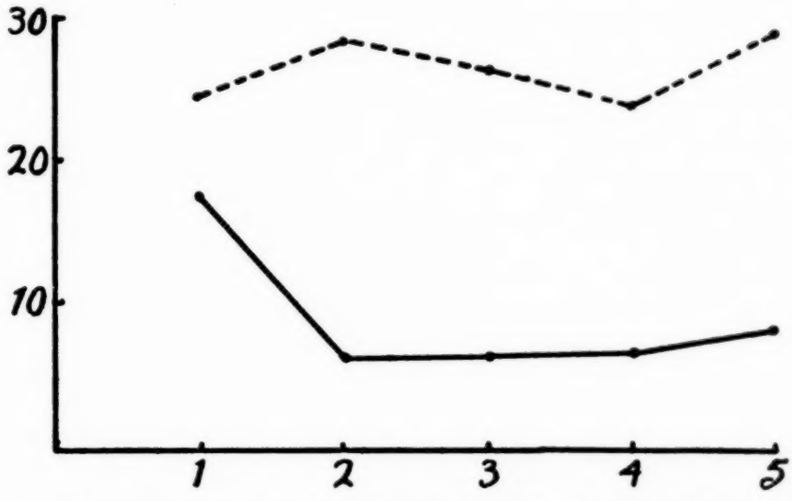

FIG. 2. Vincent curves showing the relative effectiveness of tone alone, pre-conditioning (lower curve) and tone with light (upper curve). Ordinate scale gives the mean percentage of tones effective (all subjects); abscissa, equal fractions of the stimulation series.

Effect of Paired Stimulation on Latencies and Perseveration Times.—Tone and light latencies did not change during double stimulation (Table II). Both the latency and the

TABLE II

LATENCIES OF TONE AND LIGHT FOR ALL SUBJECTS

	Mean (in sec.)	Range (in sec.)	N
Tone alone (pre-conditioning)...	.42	.12–.87	69
Tone with light.....	.42	.10–.70	480
Light alone.....	.20	.05–.45	340
Light with tone.....	.20	.05–.72	910

perseveration time became significantly longer for tone, post-conditioning, as compared with tone, pre-conditioning (Table III). The latency for tone following the paired stimulation series also was longer than that for tone with light (Table III). This longer latency approximates the interval between the beginning of the conditioned and the unconditioned stimulus. The perseveration time for the paired stimuli was less than that for light alone, pre-condi-

TABLE III

LATENCIES AND PERSEVERATION TIMES FOR TONE ALONE (PRE- AND POST-CONDITIONING) AND LATENCY FOR TONE WITH LIGHT

	Latency (in sec.)				Perseveration (in sec.)			
	M	R	S.D.	N	M	R	S.D.	N
Tone alone (pre-conditioning)	.42	.12–.87	.18	69	.61	0.0–3.0	.59	69
Tone alone (post-conditioning)	.54	.15–1.1	.22	63	1.30	0.0–6.5	1.27	63
Diff.	.12				.69			
CR	3.4				4.0			
Tone with light	.42	.10–.70	.15	480				
Tone alone (post-conditioning)	.54	.15–1.1	.22	63				
Diff.	.12							
CR	4.3							

tioning (Table IV). This indicated that the factor of adaptation was operative (7).

TABLE IV

COMPARISON OF PERSEVERATION TIME FOR LIGHT ALONE AND THAT OF LIGHT WITH TONE FOR ALL SUBJECTS

	Mean (in sec.)	Range (in sec.)	S.D.	N
Light alone (pre-conditioning)	1.8	0.1–9.8	1.5	330
Light with sound	1.3	0.1–6.5	1.0	627
Diff.	.5			
CR	5.4			

The Effectiveness of Tone, Post-conditioning.—Figure 3 shows that the increased effectiveness of tone when presented with light did not persist when tone was presented alone following the paired stimulation series. The effectiveness decreased relatively rapidly from the first to the fourth tone, beyond which it fluctuated. Although this curve was constructed from the data for all subjects it is typical of the curve for each subject.

DISCUSSION

According to traditional concepts of the conditioned response, conditioning is demonstrated when there occurs an alteration in response with respect to a neutral stimulus by virtue of its repeated presentation in a controlled relation-

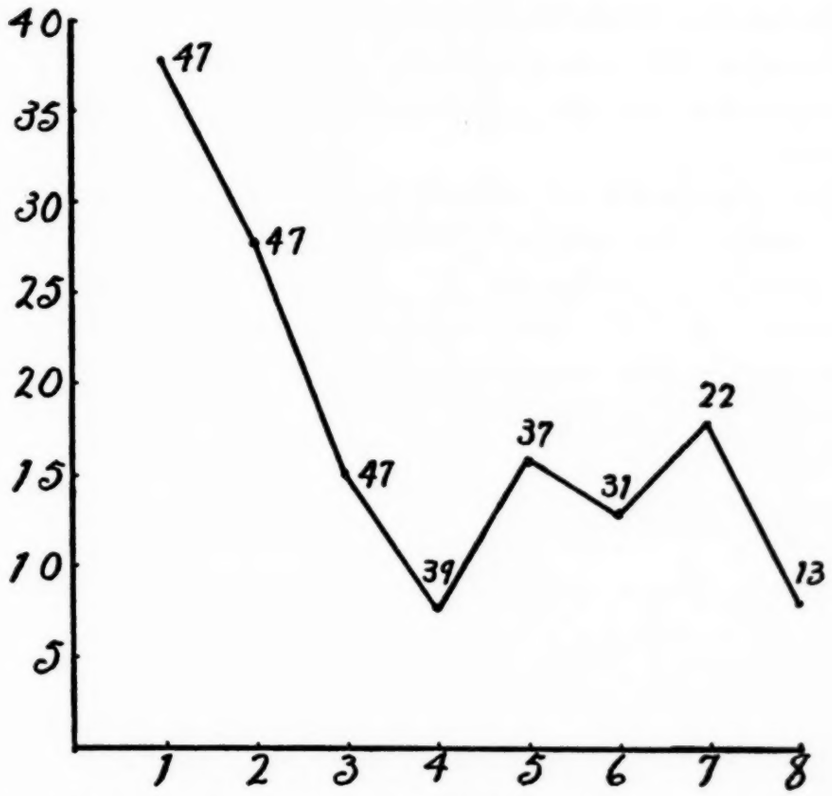

FIG. 3. Curve showing the course for effectiveness of tone alone, post-conditioning. Ordinate scale gives the percentages of tones effective; abscissa, the numerical order of tonal presentations. Since each subject was given several post-conditioning series, he could have several first presentations, several second presentations, etc. The numbers along the curve represent the total number of first presentations, second presentations, etc., for all subjects. The percentage of tones effective as given on the ordinate scale was derived from these numbers given along the curve.

ship with an originally effective stimulus. Therefore, we conclude that conditioning of the electrical response of the cortex occurred because of (1) the increased effectiveness of tone during the paired stimulation period, (2) the retention of this increased effectiveness to tone without reinforcement, and (3) the increased latency of the response to tone during the tone alone, post-conditioning series. These three findings meet the criteria for conditioning.

The increased effectiveness of tone in the paired stimulation series was maintained at a relatively high level because of continued reinforcement. When the tone was presented without reinforcement the instability of the conditioned response revealed itself in a rapid decrease of the effectiveness of tone. Furthermore, only after continued reinforcement

did the latency of the response to tone as revealed in the post-conditioning test approximately equal the interval between the beginning of the conditioned and the unconditioned stimulus.

That simultaneous stimulation of two sense modalities may change the sensory response both qualitatively and intensitively is evidenced by the work of Cason (**2**) and Hartmann (**4**). If such changes in auditory sensation occurred during the conditioning series they could not have accounted for the increased effectiveness of tone which we found (**1**).

Furthermore Kelley (**5**) reported negative results from an attempt to produce chromæsthesia by the conditioning procedure. These findings point to the conclusion that the brain wave technique is more sensitive in detecting alterations in response under certain conditions than the introspective method. This leads us to the question: How delicate are brain potentials in revealing those variables of which changes in sensation are directly a function? Three pertinent findings provide a basis for an answer to this question. First, systematic and consistent changes in sensation occur without demonstrable changes in brain potential patterning. For example, discrete tonal stimuli are effective in altering the electrical response of the cortex in only about 10 percent of the presentations (see Table I). Second, a subliminal light is effective in obliterating or depressing the alpha rhythm (**3**).[3] Third, the possible perceptual changes to tone due to its presentation with light could not have been of sufficient magnitude to account for the results obtained in the present study. From these considerations it follows first, that with the present techniques the physiological correlates of sensation in all of their significant respects are not revealed in brain potential patterning and second, that changes in the electrical activity of the cortex due to stimulation may occur without concomitant sensations. Thus the threshold for cortical changes due to stimulation is lower than that for the cortical changes identical with sensation. Finally, therefore, brain

[3] We have verified this finding.

potential patterning and sensation may vary independently of each other.

Summary and Conclusions

1. The effectiveness of tone in altering brain potential patterning when presented with light was significantly greater than that for tone alone, pre-conditioning.

2. Tone and light latencies did not change during the paired stimulation series.

3. Both the latency and the perseveration time were significantly longer for tone alone, post-conditioning, than for tone alone, pre-conditioning.

4. The latency for tone alone, post-conditioning was significantly longer than that for tone with light.

5. The perseveration time for the paired stimuli was significantly less than that for light alone, pre-conditioning.

6. The test for the effectiveness of tone without reinforcement revealed the instability of the conditioned response.

7. The conclusion is that conditioning of the electrical response of the cortex occurred under the conditions of this study.

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References

1. Bagchi, B. K., The adaptation and variability of response of the human brain rhythm, *J. Psychol.*, 1937, **3**, 463–485.
2. Cason, H., Sensory conditioning, *J. Exper. Psychol.*, 1936, **19**, 572–591.
3. Cruikshank, R. M., Human occipital brain potentials as affected by intensity-duration variables of visual stimulation, *J. Exper. Psychol.*, 1937, **21**, 625–641.
4. Hartmann, G. W., The facilitating effect of strong general illumination upon the discrimination of pitch and intensity differences, *J. Exper. Psychol.*, 1934, **17**, 813–822.
5. Kelley, E. L., An experimental attempt to produce artificial chromæsthesia by the technique of the conditioned response, *J. Exper. Psychol.*, 1934, **17**, 315–341.
6. Loomis, A. L., Harvey, E. N., and Hobart, G. A., III, Cerebral states during sleep, as studied by human brain potentials, *J. Exper. Psychol.*, 1937, **21**, 127–144.
7. Travis, L. E., and Knott, J. R., Brain potential studies of perseveration. I. Perseveration time to light, *J. Psychol.*, 1937, **3**, 97–100.

THE INFLUENCE OF ATTITUDE AND DISTRACTION

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Introduction

The purpose of the present experiment was to study the influence of distractions to which the *S*s were more or less accustomed on several psychological activities. The five psychological activities used were (*a*) cumulative addition, (*b*) paired associates learning, (*c*) arithmetical problem solving, (*d*) reading of interesting prose material, and (*e*) reading of proverbs, sayings, and jokes. The *S*s worked under two different attitudes which we shall refer to as the congenial pace and maximum effort attitudes; and they graded the extent to which they were accustomed to the distractions used, the amount of effort which they exerted during the working periods, and the difficulty of the working conditions.

Studies of Attitude and Distraction.—The changes which occur in the *S*'s attitudes towards his work in the presence of extraneous auditory stimuli have attracted wide-spread attention since Morgan's (11) important investigation. Morgan's *S*s substituted letters and numbers, and the response involved pressing on one of 10 keys with the right forefinger. The work was sometimes carried out under quiet conditions and at other times under a pandemonium of sounds which included hammers, electric bells, a fire gong, and phonograph records. Sounds influenced the time and accuracy scores to a very slight extent, and after an initial retardation the *S*s exceeded the speed made under quiet conditions, accuracy being equal under the two conditions. The fact that the keys were struck harder under the noisy conditions suggested that the *S*s exerted more effort when the sounds were present. Relatively little could be said about the efficiency of any condition on the basis of the time measurements alone, and

Morgan's conclusions were concerned with individual attitudes rather than with the externally judged efficiency of conditions. His principal conclusion was that any change in the resistance offered to maintaining a psychological set changes the energy output. In a later study, Morgan (**12**) found that the sounds of a phonograph, and a fire gong or buzzer, interfered with the formation of associations between 3-letter words and digits presented visually, the initial interference being greater than later in the working period. Muscular tension seemed greater during the noisy conditions, and less material was retained after two days when it was learned under noisy conditions. Ford (**7**) stimulated his *S*s with an automobile horn or with a humorous monologue played on a phonograph, and they selected and added single-digit numbers from rows of mixed numerals and letters. The cessation of a distracting noise seemed to be a distraction. Ford's results seemed in a general way to corroborate those of Morgan (**11**). In Weber's (**20**) study, jokes, music, and pictures interfered with learning and association, but the effect of the distractions depended upon the *S*'s attitude towards the distraction and towards the task, and the effect was least when there was a positive set to perform the task.

Vernon and Warner (**19**) studied the influence of an electric bell on the solving of arithmetical problems, and found that the most satisfactory measure of the effects of the sound was the *S*'s rating of the extent to which he was distracted. Fryer (**8**) investigated the attitudes and incentives of *S*s adding under changing auditory stimulus patterns produced by five bell tones of different pitches. Verbal reports and other records indicated that stimulus patterns must first affect the *S*'s intent to work, and that this intent to work then determines the performance. Accomplishment was influenced by the stimulus patterns that were consciously accepted, but an intent to increase speed was sometimes accompanied by emotional disturbances which temporarily decreased efficiency. Baker (**1**) found that the effect of dance music on cumulative addition was influenced by the *S*'s attitude or belief in regard to whether distractions had a

beneficial or an injurious effect on performance, and the presence or absence of music did not markedly influence the central tendency scores in the cumulative addition.

Several important investigations of distracting the attention and other conscious-verbal processes (Woodrow, **22**) have shown the desirability of taking the conscious-verbal processes of the *S*'s psychological center into consideration in all studies of the relation between attitude and distraction.

Other Studies of Distractions.—There are several other studies which are less closely related to the particular problem of the influence of attitude on the effects of distractions. Speich (**15**) studied the influence of sensory and motor distractions on word-associations, and found that the higher meaningful processes were more disturbed. Harmon (**9**) studied the slightly detrimental influence of sounds from loud phonograph records of a busy office room and of an excessively noisy street corner on the adding of 3-place numbers. Adding was ordinarily accompanied by small increases in metabolic rate, heart rate, and breathing rate and volume; but the sounds from the phonograph records increased these values during the first days of the experiment, and the effects gradually disappeared when the *S*s were in the same situation daily for several weeks. Whitely (**21**) found that music had a slightly detrimental influence on the learning of words; and Obata's (**13**) results indicated that music and noise lowered the efficiency of cancellation and addition. Cornelli (**5**) found that the efficiency of copying symbols and adding increased about 10 to 20 percent when the *S*s were stimulated by a muffled noise, a pure sound, and bits of music. The efficiency of Taylor's (**17**) *S*s in discriminating five just-noticeably-different lights and manipulating a pressure stylus was lowered by a sudden unexpected blast from an automobile horn. The decreased efficiency of discrimination and motor coördination persisted throughout the experimental period, but there was little loss of efficiency on the fourth day and on the fourth period of being stimulated by the sound.

The studies of Tinker (**18**), Hovey (**10**), and Brown (**3**) have shown that intelligence test scores are generally only

slightly influenced by extraneous auditory stimuli. The influence of distractions has been reviewed in a general way by Poffenberger (**14**, 207–228) and Bills (**2**, 500–507), and Diserens (**6**, 105–205) has given a detailed summary of the effects of music on behavior. The influence of distractions on skilled manual performances, distractibility in patients with the flight-of-ideas symptom, the use of 'suggestions by distraction' in the treatment of psychological disorders, and the use of distractions in controlling young children are not closely related to the topic of the present investigation.

It seems to be assumed in many of these studies that a distraction is any stimulus pattern which interferes with existing psychological processes, but especially with those psychological processes which the individual wishes to continue. It does not seem possible that a satisfactory analysis of distraction can be made from a purely behavioristic approach to the problem, and we have made some use of the conscious-verbal processes of the *S*s in attempting to secure a more complete analysis of the influence of the *S*s' attitudes on the effects of distracting stimuli.

General Procedure

The experiment was carried out in a small room twelve feet long and eight feet wide, and the conditions were very quiet during the no-distraction periods. Each *S* was asked to work, first, in a comfortable, congenial, natural way, without effort or strain (the congenial pace or 'C. P.' attitude); and second, each *S* was asked to work as hard as possible and to do as well as possible, and was told that his score would be compared with the scores of other *S*s (the maximum effort or 'M. E.' attitude). All of the experimental periods were six minutes long, and each of the two distraction periods was preceded and followed by a quiet period. With the C. P. attitude in the first half of the experiment, each *S* worked under quiet, then distraction, then quiet conditions; and with the M. E. attitude in the second half of the experiment, each *S* also worked under quiet, then distraction, then quiet conditions. There was a rest period of one or two minutes after each of the six 6-minute experimental periods. Each *S* served as his own control for the influence of the two attitudes and for the influence of the two working conditions. There was some warming up effect in the first period, and there was occasionally a slight fatigue in the last period.

S graded the amount of effort which he exerted during each of the six periods, using the scale: 3 = a great deal of effort, 2 = a moderate amount of effort, and 1 = very little effort; and *S* also graded the difficulty of the working conditions, using the scale: 3 = very difficult, 2 = rather difficult, and 1 = not difficult. The *S*s in the three of the experiments were required to maintain a minimum pressure of 10 pounds with the right foot on a brake pedal apparatus, and in each 6-minute period *E* recorded the

pressure being exerted by *S* at the end of two, four, and six minutes. The experiment with each *S* was completed in one hour.[1]

Experiment I

Cumulative Addition

Psychological Activity.—*E* spoke a 2-place number, and *S* added two, three, four, five, then two, three, etc., speaking the successive sums aloud. *E* followed the cumulative addition with the aid of reference number-tables, and whenever *S* made a mistake *E* told him the correct number and the number to add. *S* was given a new number every 30 seconds, and the score for each 6-minute period was the total number of additions made.

Distractions.—During the distraction periods, *E* drummed with a pencil on the table, laughed, mumbled, hummed, rattled wooden spools in a box, scraped chairs on the floor, and rustled paper. The attempt to have the *S*s believe that these sounds were unintentional was not particularly successful in the second half of the experiment. The *S*s habitually worked with others in the room and listened to radio programs while they were studying; and the laboratory situation was on the whole a fairly natural one for the *S*s.

Subjects.—Thirteen men and five women, mostly Sophomores and Juniors and relatively naive, were used as *S*s.

Influence of Conditions.—The M. E. attitude increased the number of additions, but did not change the number of errors. The Median number of additions in each period for the two attitudes and for the quiet and distracting conditions were:

	Q.	D.	Q.
C. P.	166	173	188
M. E.	197	190	197

Distractions produced a slight decrease in additions. The Median numbers of errors for each period were:

	Q.	D.	Q.
C. P.	8.5	8.5	9.0
M. E.	8.5	9.0	9.0

Distractions did not markedly influence errors.

The Subjects' Grades.—Distractions increased the *S*s' grades for effort exerted and for difficulty of the working

[1] I am under a special obligation to Ruth J. Zimmerman, Karl K. Hilgendorf, Alvin M. Bruss, Violet J. Bagley, and Karl A. Liefert who acted as experimenters in the five experiments.

conditions. The Median correlation of *S*'s grade for effort was + .62 with additions, and + .33 with errors. The Median correlation of *S*'s grade for difficulty was − .47 with additions, and + .61 with errors.

Tension.—There was a very slight decrease in the pressure on the foot pedal during the distraction period, but the *S*s tended to talk louder, their voice seemed more tense, and they were more restless. Some of the *S*s tried to show that the distractions did not interfere with their work. The Median correlation on pressure of the foot pedal was + .17 with additions and + .23 with errors.

Conclusion.—Distractions produced a slight decrease in additions, but did not markedly influence errors. Distractions increased the *S*s' grades for effort and difficulty. Grades for effort correlated + .62 with additions and + .33 with errors. Grades for difficulty correlated − .47 with additions and + .61 with errors. Pressure on the foot pedal correlated + .17 with additions and + .23 with errors.

Experiment II

Paired Associates Learning

Psychological Activity.—In each period *S* attempted to memorize in his own way a different typewritten list of 25 pairs of familiar 3-letter words (Cason, 4, 202) so that he could later speak the second word when *E* spoke the first word. *S*'s memory for the pairs was tested in mixed order at the end of each period, and his score was the number of pairs correctly recalled.

Distractions.—*E* snapped gum, hummed, whistled, scraped chairs on the floor, mumbled apologies, looked over *S*'s shoulder, walked around the room, and rattled wooden spools in a box. Most of the *S*s realized that the distractions were intentional in the second half of the experiment. The presence of *E* rather than the sounds seemed to be the chief distraction. Most of the *S*s were accustomed to having the radio on and having people around them while studying.

Subjects.—Thirteen men and five women, mostly Sophomores and relatively naive, were used as *S*s.

Influence of Conditions.—The M. E. attitude produced only a slight increase in pairs learned. The best scores were made in the quiet periods following the distraction period, and the scores made under distractions were the same as, or slightly less than, the Mean scores of the preceding and following quiet periods.

The Subjects' Grades.—Distractions increased the *S*s' grades for effort and difficulty. Using the results of the individual periods of all of the *S*s, pairs learned correlated + .77 with grades for effort and − .80 with grades for difficulty.

Tension.—The *S*s exerted about 10 percent more pressure on the foot pedal during the distraction periods. Using the results of individual periods, there was a Median correlation of + .25 between pressure on the foot pedal and pairs learned. The pressure was approximately the same during the C. P. and M. E. periods, and the small and unreliable difference was in favor of a greater pressure during the C. P. periods. Within a single period, the Mean pressure was less after four minutes than after two minutes, and less after six minutes than after four minutes.

Conclusion.—The number of pairs learned under distractions was the same as or slightly less than the number of pairs learned during the quiet periods. Distractions increased the *S*s' grades for effort and difficulty. Pairs learned correlated + .77 with grades for effort and − .80 with grades for difficulty. The *S*s pressed on the foot pedal about 10 percent harder during the distraction periods, and pressure correlated + .25 with pairs learned. Within a single period, pressure on the foot pedal decreased progressively after two, four, and six minutes.

Experiment III

Arithmetical Problem Solving

Psychological Activity.—A set of 49 arithmetical problems was used, each problem was typewritten on a separate card, and the *S*s used pencil and paper when they desired. The Median number of problems attempted by different *S*s during all six periods was 34. If an *S* thought he was spending too much time on a problem or could not solve it, he was allowed to pass on to the next problem. A trial with a single problem was completed when the *S* gave his answer, and *E* did not indicate whether the answer was correct or not. A record was made of (*a*) problems attempted, (*b*) problems solved, and (*c*) errors.

Distractions.—During the distraction period, a moderately loud radio was going continuously, *E* occasionally tapped with his pencil on the table or chair or sole of his shoe, scraped his feet several times as he walked across the room and back again twice, tapped on the table in time with the music, turned the pages of his note book twice, looked over *S*'s shoulder once, and opened and closed the top of a phonograph once. The radio was kept tuned to WIBA Madison (generally NBC), most of the *S*s came

between 7 and 9 P.M., and the experiment was carried out in Dec., 1937 and Jan., 1938. *E* gave more distracting stimuli on the few occasions when there was a short break in the radio program. The *S*s said that talking on the radio programs was much more distracting than music, news flashes and comedy sketches were the most disturbing kinds of talking, jazz music was the most distracting kind of music, especially if someone sang, and classical music hardly disturbed them at all. The majority of the *S*s were not accustomed to studying with a radio on, and they were only slightly or moderately accustomed to the other distractions.

Subjects.—Fourteen men and 11 women, mostly in the Freshman, Sophomore, and Junior classes and relatively naive, were used as *S*s.

Influence of Conditions.—The M. E. attitude definitely increased problems attempted and problems solved, and it seemed to cause a slight increase in errors. The Mean numbers of problems attempted were:

	Q.	D.	Q.
C. P.	5.6	5.1	4.9
M. E.	7.4	5.5	6.5

With the M. E. attitude, distractions decreased problems attempted. The Mean numbers of problems solved were:

	Q.	D.	Q.
C. P.	3.9	3.2	3.4
M. E.	5.3	3.6	4.5

Distractions decreased problems solved, especially with the M. E. attitude. The Mean numbers of errors were:

	Q.	D.	Q.
C. P.	1.7	1.9	1.5
M. E.	2.1	1.9	2.1

Distractions did not have any marked influence on errors.

The Subjects' Grades.—At the end of the third and sixth periods, S graded the amount of effort which he had exerted and the difficulty of the working conditions in each of the three preceding periods. The Mean grades for effort were:

	Q.	D.	Q.
C. P.	2.0	2.3	1.8
M. E.	2.1	2.6	1.9

The grades for effort were higher during the distraction periods. The Mean grades for difficulty were:

	Q.	D.	Q.
C. P.	1.3	2.3	1.2
M. E.	1.5	2.6	1.2

The grades for difficulty were much higher during the distraction periods. Using the grades and scores of each of the six periods and calculating the correlations for individual Ss, the Median correlations were:

	Problems Attempted	Problems Solved	Errors
Grades for effort	−.09	−.12	+.42
Grades for difficulty	−.12	−.05	+.15

There was a slight negative relation between the Ss' grades for effort and difficulty, on the one hand, and problems attempted and problems solved, on the other. Errors correlated much higher with grades for effort than with grades for difficulty.

Conclusion.—Distractions decreased problems solved, but did not have a marked influence on errors. The grades for effort were higher and the grades for difficulty were much higher during the distraction periods. Grades for effort correlated − .09 with problems attempted and − .12 with problems solved; and grades for difficulty correlated − .12 with problems attempted and − .05 with problems solved. Errors correlated much higher with grades for effort (+ .42) than with grades for difficulty (+ .15).

Experiment IV

Reading of Interesting Prose Material

Psychological Activity.—The *Ss* read silently and were asked to read with understanding in Stevens' (**16**) book on *Paul Bunyan*, beginning on page 132 at the beginning of the whole experimental period. The Median number of pages read by the different *Ss* during the whole experiment was eight. A record was made of pages and fractions of pages read during each of the six periods.

Distractions.—During each distraction period, a moderately loud radio was going continuously, *E* drummed on the table with his fingers and tapped on the table with his pen at intervals, shuffled his feet once or twice, scraped a chair on the floor twice, rustled papers once, and walked across the room and back again once. The conditions of the radio distractions were the same as in Experiment III except that most of the *Ss* came between 2:30 and 5:30 P.M., and between 7 and 9 P.M. The large majority of the *Ss* were not accustomed to studying or reading with as much noise as was present during the distraction periods of this experiment.

Subjects.—Fifteen men and 10 women, mostly 18, 19, and 20 years of age, in the Freshman, Sophomore, and Junior classes, and relatively naive, were used as *Ss*.

Influence of Conditions.—The M. E. attitude definitely increased pages read. With the C. P. attitude, more pages were read during the distraction period than during the preceding quiet period, but fewer pages were read during the distraction period than during the following quiet period, pages read during the distraction period being the same as the Mean of the two quiet periods. With the M. E. attitude, pages read during the distraction period were slightly less than during the quiet periods.

The Subjects' Grades.—At the end of the third and sixth periods, *S* graded the amount of effort which he had exerted and the difficulty of the working conditions during each of the three preceding periods. The Mean grades for effort with the two attitudes and under the two conditions were:

	Q.	D.	Q.
C. P.	1.8	2.6	1.6
M. E.	1.8	2.6	1.6

The grades for effort were much higher during the distraction periods. The Mean grades for difficulty were:

	Q.	D.	Q.
C. P.	1.3	2.4	1.1
M. E.	1.2	2.4	1.2

The grades for difficulty were much higher during the distraction periods. Using the grades and scores of each of the six periods and calculating the correlations for individual *S*s, the Median correlation of pages read was − .02 with grades for effort and − .06 with grades for difficulty.

Conclusion.—Distractions seemed to have a very slight detrimental influence on pages read. The grades for effort and difficulty were much higher during the distraction periods. Pages read correlated − .02 with grades for effort and − .06 with grades for difficulty.

Experiment V

Reading of Proverbs, Sayings, and Jokes

Psychological Activity.—*S* read silently in a 35-page set of typewritten material composed of philosophic sayings having a subtle meaning and jokes of a more or less obvious character. An attempt was made to obtain material that would be as interesting as possible to the *S*s. *S* was instructed to read the material attentively and not to leave an item until it was clearly understood. A record was made of the number of items read.

Distractions.—*E* walked around the room, scraped chairs on the floor, chewed gum, whistled off key, stood behind *S*, and tinkered with apparatus. The *S*s tried to show *E* that the distractions did not interfere with their work. Most of the *S*s were accustomed to reading with the radio on and with people in the room.

Subjects.—Fourteen men and one woman, 19, 20, or 21 years of age and relatively naive, were used as *S*s.

Influence of Conditions.—The M. E. attitude definitely increased the number of items read. With the C. P. attitude, items read were less under distractions than in either of the quiet periods; and with the M. E. attitude, items read were considerably less under distractions than in either of the quiet periods. With both attitudes, the best performance occurred in the quiet period following the distraction period.

The Subjects' Grades.—With both attitudes, the grades for effort were higher during the distraction period than during either of the quiet periods, and these grades were lowest in the quiet period following the distraction period. With both attitudes, the grades for difficulty were higher during distrac-

tions than during either of the quiet periods, and these grades were slightly lower in the quiet period following the distraction period than in the quiet period preceding the distraction period. The combined results of all of the periods for all of the *S*s show that when the *S*s thought they were exerting a great deal of effort they did not read any more items than when they thought they were exerting a moderate amount of effort or very little effort. The grades for difficulty correlated very low with items read, but + .80 with grades for effort.

Tension.—With both attitudes, the *S*s pressed harder on the foot pedal during the distraction period than during either of the quiet periods. Using the six periods of individual *S*s, a Median correlation of zero was obtained between the amount of pressure exerted and items read. The *S*s pressed harder on the foot pedal when they thought they were exerting a great deal of effort than when they thought they were exerting a moderate amount of effort or very little effort. Most of the *S*s denied that the pressure on the foot pedal was itself a distraction.

Conclusion.—Distractions decreased items read, especially with M. E. attitude. The grades for effort and difficulty were higher during the distraction periods. The *S*s did not read any more items when they thought they were exerting a great deal of effort than when they thought they were exerting a moderate amount of effort or very little effort. Grades for difficulty correlated very low with items read, but grades for difficulty correlated + .80 with grades for effort. The *S*s pressed harder on the foot pedal during the distraction periods and when they thought they were exerting a great deal of effort, but pressure on the foot pedal correlated zero with items read.

Summary of Results and General Conclusion

The principal results of the five experiments have been brought together for convenience in Table 1.

Influence of the M. E. Attitude.—The M. E. attitude produced an increase in efficiency in all five experiments, but the increase was small in paired associates learning. The

TABLE I

SUMMARY OF RESULTS

	Cumulative Addition: No. of		Paired Associates Learning: No. of Pairs Learned	Arithmetical Problem Solving: No. of			Reading of Interesting Prose Material: No. of Pages Read	Reading of Proverbs, Sayings, and Jokes: No. of Items Read
	Additions	Errors		Problems Attempted	Problems Solved	Errors		
Influence of M. E. attitude.....	Increase	No change	Slight increase	Definite increase	Definite increase	Slight increase	Definite increase	Definite increase
Influence of distractions (C. P. and M. E.).....	Slight decrease	No marked influence	No influence, or slight decrease	Decrease with M. E. attitude	Decrease, especially with M. E. attitude	No marked influence	Slight decrease	Decrease, especially with M. E. attitude
Influence of distractions on grades for effort.....	Increase		Increase	Increase			Large increase	Increase
Influence of distractions on grades for difficulty.....	Increase		Increase	Large increase			Large increase	Increase
Correlation with grades for effort	+.62	+.33	+.77	−.09	−.12	+.42	−.02	Low
Correlation with grades for difficulty	−.47	+.61	−.80	−.12	−.05	+.15	−.06	Very low
Influence of distractions on pressure on foot pedal	Slight decrease		Increase of 10 percent	——			—	Increase
Correlation with pressure on foot Pedal	+.17	+.23	+.25	—	—	—	—	Zero

M. E. attitude did not affect errors in addition, and caused only a slight increase in errors in paired associates learning.

Influence of Distractions.—Distractions produced a slight decrease in efficiency in addition, paired associates learning, and reading of prose material. Distractions caused a decrease with the M. E. attitude in problems attempted, and a decrease especially with the M. E. attitude in problems solved and the number of proverbs, etc. read. Distractions had no marked influence on errors in addition and problem solving. Results not given in the table showed that the quiet period following the first distraction period was on the whole the most efficient.

The Subjects' Grades.—Distractions increased the *Ss'* grades for effort and difficulty in all five experiments. Grades for effort correlated positively with efficiency in addition and problem solving, but these grades correlated low or negatively with efficiency in problems attempted, problems solved, and the two recreational activities. Grades for effort correlated positively with errors in addition and problem solving. Grades for difficulty correlated negatively with efficiency in addition and paired associates learning; very low or negatively with efficiency in problems attempted, problems solved, and the two recreational activities; positively with errors in addition and positively but low with errors in problem solving.

Tension.—Distractions had only a slight influence on the pressure on the foot pedal. Pressure on the foot pedal correlated positively but low with addition, errors in addition, and paired associates learning; and zero with reading of proverbs, etc.

Individual Differences.—In the results described above, we have been principally concerned with general trends and central tendencies, but in practically every function studied, there were large individual differences between different *Ss* and in the same *S* at different times.

General Conclusion.—A study was made of the influence of several common distractions on cumulative addition, paired associates learning, arithmetical problem solving, and two recreational reading activities. The general effect of distractions was to make the conditions more difficult to work under and to lower efficiency. The *Ss* already had a set to

work, and distractions caused them to exert greater effort. In spite of the greater effort, efficiency during distractions was lower than during the quiet periods. The Ss' grades for effort exerted, difficulty of the working conditions, and the extent to which they were accustomed to the distractions were of definite value in themselves and in the interpretation of other results. Pressure on the foot pedal was on the whole poorly correlated with other data.

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REFERENCES

1. BAKER, K., Pre-experimental set in distraction experiments, *J. Gen. Psychol.*, 1937, **16**, 471–488.
2. BILLS, A. G., *General experimental psychology*, 1934.
3. BROWN, A. W., *et al.*, The influence of distraction upon mental-test performance, *Psychol. Clin.*, 1934, **22**, 213–219.
4. CASON, H., Specific serial learning: a study of backward association, *J. Exper. Psychol.*, 1926, **9**, 195–227.
5. CORNELLI, G., *Psychol. Abstracts*, 1937, No. 3588.
6. DISERENS, C. M., *The influence of music on behavior*, 1926.
7. FORD, A., Attention-automatization: an investigation of the transitional nature of mind, *Amer. J. Psychol.*, 1929, **41**, 1–32.
8. FRYER, D., A genetic study of motivation under changing auditory situations, *Brit. J. Psychol., Gen. Sect.*, 1934, **24**, 408–433.
9. HARMON, F. L., The effects of noise upon certain psychological and physiological processes, *Arch. of Psychol.*, 1933, **23**, No. 147.
10. HOVEY, H. B., Effects of general distraction on the higher thought processes, *Amer. J. Psychol.*, 1928, **40**, 585–591.
11. MORGAN, J. J. B., The overcoming of distractions and other resistances, *Arch. of Psychol.*, 1916, No. 35.
12. MORGAN, J. J. B., The effect of sound distraction upon memory, *Amer. J. Psychol.*, 1917, **28**, 191–208.
13. OBATA, J., *et al.*, The effects of noise on human efficiency, *J. Acous. Soc. Amer.*, 1934, **4**, 255–261.
14. POFFENBERGER, A. T., *Applied psychology, its principles and methods*, 1927.
15. SPEICH, R., Reproduktion und psychische Aktivität, *Arch. f. d. ges. Psychol.*, 1927, **59**, 225–338.
16. STEVENS, J., *Paul Bunyan*, 1925.
17. TAYLOR, J. H., Responses to startle stimulation, *J. Gen. Psychol.*, 1935, **12**, 108–131.
18. TINKER, M. A., A study of the relation of distracted motor performance to performance in an intelligence test, *Amer. J. Psychol.*, 1922, **33**, 578–583.
19. VERNON, H. M., AND WARNER, C. G., Objective and subjective tests for noise, *Person. J.*, 1932, **11**, 141–149.
20. WEBER, H., Untersuchungen über die Ablenkung der Aufmerksamkeiten, *Arch. f. d. ges. Psychol.*, 1929, **71**, 185–260.
21. WHITELY, P. L., The influence of music on memory, *J. Gen. Psychol.*, 1934, **10**, 137–151.
22. WOODROW, H., The measurement of attention, *Psychol. Monog.*, 1914, **17**, No. 76.

STUDIES IN RETROACTIVE INHIBITION: XI. THE INFLUENCE OF THE RELATIVE SERIAL POSITIONS OF INTERPOLATED SYNONYMS IN TWENTY-ITEM LISTS

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Problem

The inhibitory influence of interpolated synonyms upon the corresponding adjectives in the original lists was found in a previous experiment [1] to be independent of the relative serial positions of adjective and synonym. When each adjective in the interpolated list was a synonym of the adjective in the corresponding position in the original list, interference effects were large. Displacement of the synonyms two or four positions forward with reference to their corresponding adjectives in the original list produced no important change in amount of interference. Reversal of the serial order of the interpolated synonyms with reference to the adjectives of the original list failed to produce any important change, as did, also, presentation in two different random orders. Synonyms of the adjectives in the original list need, then, only to be present somewhere in the interpolated list; the relative serial positions of adjectives and synonyms do not matter.

These results imply a set of inter-list relations in which serial position is not a major common term. They imply, further, a complexity and fluidity of action of list on list which is significant for association theory, quite aside from the immediate problems of retroactive inhibition.

The present experiment has repeated certain fundamental conditions of the former one in order to find whether similar

[1] J. A. McGeoch and G. O. McGeoch. Studies in retroactive inhibition: VI. The influence of the relative serial positions of interpolated synonyms. *J. Exper. Psychol.*, 1936, **19**, 1–23.

results will appear when longer lists are employed. The lists in the earlier experiment were 10 items long. It is possible, however, that in a longer list disparity of relative serial position will have some influence upon degree of inhibition, since there the absolute amount of disparity introduced will be greater. This experiment has used lists of 20 items. If lists of 10 and of 20 items give different results, interpretation must take account of the limiting factor of length of list. If the two lengths give similar results, the generality of the results and of their interpretation will be considerably extended.

PROCEDURE

Lists of 20 two-syllable adjectives, plus an initial word which served as a cue and was not learned, were exposed at a 2-sec. rate on an electrically driven drum and were learned by the anticipation method. Prior to the beginning of the experiment all subjects were given 3 practice sessions. On the first practice day one list was learned to a criterion of 3 perfect trials in succession and was relearned to the same criterion. On the second and third practice days this was repeated with different lists and in each case an interpolated list was given 12 repetitions during the interval between learning and relearning. The subject came to the main experiment, therefore, having had prior practice under one rest and two work conditions analogous to those used in the experiment.

All original lists were given 10 presentations, followed by a 12-min. interval[2] and then by relearning to 3 successive perfect repetitions. The 5 conditions of the experiment represent as many variations of the activity engaged in by the subject during the interval. Other conditions are constant. Under the *rest condition* the subjects read jokes throughout the interval under instruction to pick the best of those read. Under the four others, all of which are *work conditions*, the subjects were given 12 repetitions of a second or interpolated list, followed by reading jokes during the remainder of the interval. The interpolated learning began 30 sec. after the completion of the tenth repetition of the original list. The 4 work conditions differ from each other only in the character or in the arrangement of the adjectives in the interpolated lists.

The interpolations are described in the following enumeration. (1) Each adjective in the interpolated list was a synonym of the adjective in the corresponding position in the original list. (2) Each adjective in the interpolated list was a synonym of an adjective in the original list but appeared in a position four places removed in the forward direction from the position of that adjective in the original list.[3] (3) Each adjective in

[2] It was necessary to use a 12-min. interval, instead of the 10-min. interval previously employed, because the twelve presentations of a twenty-item list required slightly more than 10 min. and, in order to make conditions comparable, there should be a rest interval between the interpolated learning and the beginning of the relearning of the original list. The lengthening of the interval by 2 min. cannot have affected seriously the comparability of the two experiments.

[3] The implications of the placing of the synonyms under this condition, where the last 4 synonyms must be displaced to the first 4 positions in the list, have been sufficiently discussed on pages 3 and 4 of the earlier paper.

the interpolated list was a synonym of one in the original list, but the synonyms were arranged in a random order with reference to the corresponding adjectives in the original list. (4) The interpolated list was composed of adjectives unrelated in meaning to those of the original list.

This experiment has repeated with twenty-item lists 4 of the fundamental conditions of the earlier experiment. A comparison of Work 1, in which the synonyms occupy the same positions as their corresponding adjectives, and Work 4, in which the interpolated adjectives are unrelated to the original adjectives, will tell us whether the relative inhibitory potencies of synonyms and unrelated adjectives are altered by the use of the longer list. These two conditions provide reference points with which to compare the two other work conditions. Work 2, in which the inter-list relations are systematically varied, and Work 3, in which they are varied randomly, are representative examples of the inter-list relations previously studied.

The synonyms used in the interpolated lists were selected by 10 judges in the same way as in the earlier experiment. Ten lists of 20 two-syllable adjectives were constructed, five of which contained synonyms of the other five. Each list of synonyms was arranged in each of the 3 ways required by Work 1, 2, and 3. The lists learned under Rest and under Work 4 were chosen from the 10 lists, and by means of a counterbalancing scheme the lists were distributed over the conditions in a manner designed to free the results from the influence of differences between lists. The 5 conditions were gone through in a counterbalanced practice order.

Thirty male college students went through each of the 5 conditions. All subjects were, prior to the learning of the first practice list, unfamiliar with both the anticipation method and other forms of learning in the laboratory.

Something should be said at this point concerning our choice of the frequencies of presentation of the original and interpolated lists. In the earlier experiment the original lists were given a constant frequency of 5 repetitions and the interpolated lists a constant frequency of 10. We wished to obtain equivalent degrees of learning in the present experiment. There are no fixed standards in terms of which degrees of learning may be compared, but we have selected as one useful measure the number of correct anticipations on the last learning trial, and have assumed that lists of 10 and 20 items may be considered to be learned to an equivalent degree if the proportions of correct anticipations on the last learning trial are equal. This is probably a better measure here than is mean correct anticipations on all trials. Preliminary experimentation showed that, under these conditions, frequencies of 10 and 12 trials with twenty-item lists would yield approximately twice as many correct anticipations on the last trial as would frequencies of 5 and 10 trials with ten-item lists. Table I shows the degree to which this has been the case in the data of the experiment. Specific comparisons of any two conditions in the table are unimportant. The point in question is whether the proportion of correct anticipations in the two experiments tends to be the same, that is, whether the means in the present experiment tend to be twice those of the 1936 experiment. There are, as we realize, difficulties with such a standard as this, but so long as we rely on it only as one statement of the relative degrees of learning in the two experiments, no error can be introduced by it.

There are variations, but the means in the present experiment are roughly twice those of the 1936 experiment and it may be concluded that in terms of the measure used the frequencies employed in the two experiments have yielded degrees of learning which are sufficiently equivalent for our purpose. Equivalence in degree of learning is not, insofar as is known, essential to the major problem, but if the results of the two experi-

ments do not agree there would remain the possibility that the disagreement was a function of the frequencies; and, methodologically, a duplication of the degrees of learning is, in any case, desirable.

TABLE I

MEAN CORRECT ANTICIPATIONS ON THE LAST TRIALS OF ORIGINAL AND INTERPOLATED LISTS

Condition	1936 Experiment		Present Experiment	
	Original Lists	Interpolated Lists	Original Lists	Interpolated Lists
Rest	6.5		13.6	
Work 1	5.6	8.6	13.4	16.2
Work 2	6.0	7.8	13.7	17.1
Work 3	5.7	7.8	14.0	16.8
Work 4	6.2	8.1	12.8	16.8
Work 5	6.5	8.1		

The amounts learned during the 10 presentations of the original lists and during the 12 presentations of the interpolated lists have also been measured in terms of the total numbers of correct anticipations. Retention has been measured in terms of 4 different scores: (1) Recall 1, which is the number of correct anticipations on the first relearning trial; (2) Recall 2, which is the number of correct anticipations on the second relearning trial; (3) Relearning 1, which is the number of trials required to relearn to the first perfect repetition; and (4) Relearning 2, which is the number of trials required to relearn to the criterion of 3 successive perfect trials. The number of presentations during the learning period is constant and it is, therefore, unnecessary to compute saving scores.

RESULTS

Comparisons of Degrees of Learning.—All of the original lists were given 10 learning presentations and all of the interpolated lists were given 12, but it is desirable to check this constant frequency by other measures in order to have as complete a statement as possible of the degrees of learning. Reference to Table I shows that, in terms of the number of correct anticipations at the last learning trial, the conditions of this experiment do not differ importantly.[4] The mean number of correct anticipations per subject offers an additional measure by which frequency may be checked. The mean numbers per subject, with their sigmas, are given

[4] The sigmas of the means for the original lists are .55, .59, .60, .55, and .63 under the 5 conditions respectively. No difference is statistically reliable. The sigmas of the means for the interpolated lists are .35, .47, .42, and .50 for the 4 work conditions, respectively. Again no difference is statistically reliable.

TABLE II

MEAN NUMBERS OF CORRECT ANTICIPATIONS ON ALL LEARNING TRIALS

Condition	List			
	Original		Interpolated	
	Mean	σ_M	Mean	σ_M
Rest	78.6	4.3		
Work 1	75.1	3.8	126.2	4.2
Work 2	76.7	4.6	123.4	4.6
Work 3	78.7	4.1	129.3	4.3
Work 4	77.3	4.2	124.7	4.9

in Table II. The differences are small, exhibit no regular tendency, and are statistically unreliable. It may be concluded that any differences in retention which may be found have not been importantly influenced by differences in measured degree of learning.

The critical reader will note that the addition of two more presentations to the interpolated list than to the original has greatly increased the mean number of correct anticipations. This is to be accounted for mainly by the high positive transfer from the original list to the interpolated. We have counted the correct anticipations trial by trial in each case and have found that the totals are approximately the same at the second trial (the first on which anticipations are possible) in the two cases, but that they are considerably higher at all succeeding trials. When we add to this the fact that by the tenth trial the subjects are correctly anticipating a large percentage of the items and that the two additional repetitions of the interpolated list mean a large increase in the absolute number of correct anticipations, the difference between original and interpolated lists can be readily understood.

Measures of Retention and of Retroactive Inhibition.—The means of the recall and relearning scores, with the sigmas of the means, are presented in Table III. It is unnecessary to compute percentages of retroactive inhibition since the conditions under which the learning was done are the same throughout and since the degrees of original learning are

equivalent. Each work condition is to be compared with the rest condition.

TABLE III

MEAN RECALL AND RELEARNING SCORES

	Recall 1		Recall 2		Relearning 1		Relearning 2 [5]	
	M	σ_M	M	σ_M	M	σ_M	M	σ_M
Rest	10.90	.51	12.57	.63	9.47	.86	15.93	1.78
Work 1 (Syn. same pos.)	2.87	.38	6.47	.61	13.30	1.50	19.87	1.93
Work 2 (Syn. 4 pos. rem.)	3.73	.51	7.73	.68	12.40	1.33	17.30	2.31
Work 3 (Syn. random ord.)	4.33	.54	9.50	.68	11.77	1.21	16.70	1.50
Work 4 (Unrelated adj.)	4.83	.51	9.20	.65	11.23	.56	14.77	1.46

[5] The mean relearning trials do not include the criterial trials.

It is at once apparent that each interpolated list has produced a decrement in retention by every one of the four measures, with the single exception of the unrelated adjectives when retention is measured in terms of Relearning 2. The decrement is greatest in terms of Recall 1 and least in terms of the relearning scores. Interpolation of unrelated adjectives produces a smaller decrement in terms of Recall 1 and of Relearning 1 and 2 than does any other interpolation.[6] It is doubtful if its failure to produce a smaller decrement in terms of Recall 2 than does the interpolation of synonyms in a random order is more than a statistical accident. This experiment may, therefore, be regarded as corroborating the earlier one and other work which has compared the inhibitory effects of synonyms and unrelated adjectives.

The major problem of the present experiment is the relationship between changes in the relative serial positions of the interpolated synonyms and variations in amount of retroactive inhibition. In Work 1 the relative positions of the adjectives in the original list and their synonyms in the interpolated list are the same; in Work 2 the synonyms are displaced 4 positions forward; and in Work 3 the synonyms are in a random order with reference to their corresponding ad-

[6] The differences between the means of Work 4 and of the other work conditions are not statistically reliable, but they are consistent in direction and their direction is the same as that found in preceding experiments. It can scarcely be doubted that the tendency represented is trustworthy.

jectives in the original list. If relative serial position is an important condition of the inhibitory effects of the interpolated synonyms, a decrease in amount of interference from Work 1 to Work 3 would be expected. In the preceding experiment no uniform diminution of interference appeared with increasing disparity of relative position. In the present experiment Recall 1 and Recall 2 show a small but consistent increase from Work 1 to Work 3. Relearning 1 and Relearning 2 show a corresponding decrease. Almost no difference in any of these cases is statistically reliable and the largest differences are relatively small.[7] The consistency of the direction of the differences is their chief guarantee. If we give to this consistency the weight which it seems to deserve, the safest conclusion is that with lists of 20 adjectives there is a tendency for inhibition to decrease, and to approach that produced by unrelated adjectives, as the relative disparity between the serial positions of the interpolated synonyms and their analogues in the original list increases. Fractionation of the data, moreover, yields tendencies which confirm the tendency of the total data.

The influence of relative serial position of interpolated synonyms is, therefore, a function of the length of the lists employed. When the lists contain 10 items, inhibitory potency is independent of the relative positions of the synonymous adjectives in the two lists. When, however, the lists contain 20 items, there is a small but consistent tendency for inhibitory potency to diminish as the disparity of relative serial position increases.

Serial Position Effects in Learning.—It is possible that the subjects in some way adjusted their learning of the interpolated lists under Work 2 and Work 3 to correspond to recognized shifts in the relative serial positions of the synonyms. Evidence for this may be sought in the serial position curves of the interpolated lists. We have drawn serial position curves, in terms of numbers of correct anticipations, for both original and interpolated lists and have systematically compared them. The curves for interpolated learning have,

[7] The difference between the Recall 2 scores under Work 1 and Work 3 is three times the sigma of the difference.

under each condition, the same form as do those for the corresponding original learning. Variations, of course, appear, but there is no consistent tendency in any of the four work conditions and the only justifiable conclusion is that the serial position effects in the four conditions are essentially the same.

Seeking a compact method of reporting these relations, we have correlated by the rank-difference method the serial position values of the original and interpolated lists. The coefficients for the four work conditions are 0.71, 0.78, 0.82, and 0.85, respectively. These coefficients are frequently lowered by very small differences from list to list. Such differences are, of course, sufficient to determine the rank orders.[8]

Summary

We have found that with lists of 20 items there is a consistent tendency for inhibitory potency to diminish as disparity of relative serial position increases. In a previous experiment with lists of 10 items, it was found that inhibition did not vary with the positions of the interpolated synonyms in relation to their analogues in the original list. The influence of the relative serial positions of interpolated synonyms is, thus, a function of the number of items in the two lists.

(Manuscript received February 1, 1938)

[8] We have, in addition to these correlations, tabulated the number of items incorrectly recalled during the relearning of the original list which are transfers from the interpolated list. There is a considerable number of such intrusions, but they are not distributed over the serial positions in a way to imply any specificity to serial position.

THE EFFECT OF VARIATION IN THE DOSE OF BENZEDRINE SULFATE ON THE ACTIVITY OF WHITE RATS

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Introduction

In a previous study made by the present authors (**I**), it was found that a 3 mg./kg. dose of benzedrine sulfate, injected subcutaneously, markedly affects the temporal distribution of the white rat's spontaneous activity. The redistribution effected by the drug is characterized by an increase in activity, lasting for approximately four hours following injection, which then is followed by a decrease for the remainder of the daily period. During this initial 4-hour period of high activity the number of revolutions run by the rats averages about fifty percent of the total of a normal 24-hour day. This amount is considerably greater than for pre-feeding activity, which has been shown often to be the most active period in the daily cycle of the white rat.

At the time of our experiment no information was available concerning the therapeutic dose of benzedrine for rats, and the 3 mg./kg. amount was roughly determined on the basis of overt behavior changes noted immediately following the administration of the drug. These changes consisted mainly of head swaying and rooting movements in the sawdust of the stationary cages. Because of the unreliability of this observation method, and because wide individual differences were noted in the overt behavior changes, it was questionable whether the 3 mg./kg. was the optimal dose for rats. The present study was undertaken to determine if there is a differential effect of benzedrine depending upon the amount of the dose, and to investigate individual differences in susceptibility to the drug with reference to spontaneous activity.

Methods and Procedures

The experimental animals were fifteen white rats used in the preceding study (Searle and Brown, (1)). There was no need for subjecting them to a preliminary adaptation period inasmuch as they had already spent 61 days in the activity cages and had become accustomed to the injection procedure. Their ages at the beginning of the experiment ranged from 164 to 177 days. Since the experiment ran for a period of 24 days, all animals were tested between the ages of 164 and 201 days.

The revolving cages have been described in the previous report. Water was always kept in the cages, and the rats were fed once a day, at 5:00 P.M., with the standard laboratory diet. A small ball of food was placed in the cage and the rat allowed forty-five minutes for eating.

Pure benzyl methyl carbinamine sulfate, dissolved in physiological salt solution, was given in subcutaneous injections. Three solutions, of .3, .6, and .9 mg. per c.c. were used. Each animal received ½ c.c. of solution per 100 grams of weight. The animals were divided into three groups of five rats each. Each group received a different dose the first experimental period and then thereafter the dose was varied in a systematic way in order to reduce to a minimum the carrying over of the effect of any one dose upon that of another. The doses were 1½ mg./kg., 3 mg./kg., and 4½ mg./kg. They were so rotated that each dose was given to some of the rats each day. The 4-day sequence of rest, salt, rest, and drug was utilized, which meant that every day there were some rats resting, some receiving injections of salt solution, and some receiving benzedrine, each dose being used on at least one animal. The experiment was continued until each rat had received each dose twice.

Results with Discussion

1. *Effect on Activity of Variation in the Dose*

Table I, columns two, four and six, presents the average activity for each rat for each of the three doses for the 4-hour period from 9 A.M. to 1 P.M. The average effectiveness of the drug is greatest for the 3 mg. dose and least for the 4½ mg. dose, the means being 1½ mg. 844, 3 mg. 925, 4½ mg. 829. The records for the 8-hour period from 9 A.M. to 5 P.M. show the same general relationships, the average activity for the three doses being 1½ mg. 1366, 3 mg. 1388 and 4½ mg. 1338. None of these differences is statistically reliable. It would appear from these findings that, within the limits of the doses used, variation in the amount of benzedrine injected has no differential effect on the rat's activity. This conclusion, however, should be postponed until a further analysis is made The activity record based on the revolving drum method is dependent upon the running behavior of the animals. Any change in the rat's behavior resulting from benzedrine which inhibits running activity is not going to be reflected in terms

TABLE I

AVERAGE ACTIVITY PER RAT FOR EACH OF THREE DOSES OF BENZEDRINE FOR THE EXPERIMENTAL PERIOD FROM 9 A.M. TO 1 P.M.

Number of Rat	1½ mg./kg. Dose		3 mg./kg. Dose		4½ mg./kg. Dose	
	Number of Revolutions	Percent Increase Above Resting Level	Number of Revolutions	Percent Increase Above Resting Level	Number of Revolutions	Percent Increase Above Resting Level
1	663	77	1236	143	1366	158
2	656	273	520	216	402	168
3	577	196	611	208	702	239
4	715	87	873	167	823	106
5	624	188	710	214	791	238
6	514	93	732	132	719	130
7	561	67	1222	145	1436	171
8	260	53	440	89	169	34
9	987	217	1562	343	1488	327
10	2672	185	2316	161	1688	117
11	1843	226	1001	123	558	68
12	548	334	690	421	473	288
13	692	104	340	51	268	40
14	560	63	948	106	992	111
15	795	211	677	180	559	149

of revolutions of the cage, thus an accurate measurement of the drug's effect is prevented. It was noted in the preliminary experiment for the determination of the dosage that very large amounts of the drug produced an excessive amount of localized movement in the region of the head. The animal's head would move up and down and from side to side for long periods of time during which there was very little locomotion. As the effect of the drug wore off more running behavior was manifest. Small doses tended to cause less of this head movement and more running activity.

A further analysis of the data shows that, in terms of the revolving drum method of measuring activity, the optimal dose varies with different animals. It so happened that five animals showed a maximum effect from each of the three doses. The average activity record for optimal doses for the 4-hour period is presented in Table II. It will be observed that the 3 mg./kg. dose again produces the greatest increase in activity, the means for the three doses being as follows: 1½ mg. 859, 3 mg. 1332 and 4½ mg. 1057 revolutions, respec-

tively. These figures are not to be considered statistically reliable because of the small number of cases comprising each group.

TABLE II

AVERAGE NUMBER OF REVOLUTIONS COMPUTED FOR THE THREE DOSES FROM THE PERFORMANCE OF RATS SHOWING OPTIMAL EFFECTS. BASED ON 4-HOUR EXPERIMENTAL PERIOD

1½ mg./kg. Dose		3 mg./kg. Dose		4½ mg./kg. Dose	
Number of Rat	Number of Revolutions	Number of Rat	Number of Revolutions	Number of Rat	Number of Revolutions
4	873	2	656	1	1366
6	732	10	2672	3	702
8	440	11	1843	5	791
9	1562	13	692	7	1436
12	690	15	795	14	992
Average	859		1332		1057

In order to eliminate the possibility that the relative effect of the different doses might be conditioned upon the general activity level of the rat, the apparent differences then being attributable to the fact that a given group might contain a disproportionate number of active or inactive animals, the effect of the dose was computed as a percent increase in activity above the resting activity level of the rat. The resting activity was taken as the average 4-hour activity on the 24-hour period just preceding the drug and salt days, that is, the activity on all rest days and the nights following these days expressed as the average activity of a 4-hour period. The activity thus computed as a percentage of the resting activity is presented in Table I, columns 3, 5, and 7. The 3 mg./kg. dose is seen to produce the greatest increase in activity, the 4½ mg. dose giving the least relative increase. The mean percent increases are as follows: 1½ mg. 316, 3 mg. 360 and 4½ mg. 312. This evidence supports the finding of the first analysis based on the actual number of revolutions of the cages.

If, on the basis of the percent increase in activity, the rats are separated into groups according to the dose producing the maximum increase, the results of Table III are obtained.

TABLE III

AVERAGE PERCENT INCREASE IN ACTIVITY COMPUTED FOR THE THREE DOSES FROM THE PERFORMANCE OF RATS SHOWING MAXIMAL INCREASES. BASED ON PERIOD FROM 9 A.M. TO 1 P.M.

1½ mg./kg. Dose		3 mg./kg. Dose		4½ mg./kg. Dose	
Number of Rat	Percent Increase Above Resting Level	Number of Rat	Percent Increase Above Resting Level	Number of Rat	Percent Increase Above Resting Level
2	546	4	334	1	326
10	370	6	264	3	478
11	452	8	178	5	476
13	208	9	686	7	342
15	422	12	842	14	222
Average	400		461		367

Each dose is seen to produce maximal increases in 5 animals. These percentages indicate that the 3 mg. dose again produces the greatest increase and the $4\frac{1}{2}$ mg. dose the least.

The above findings clearly indicate that the activity cage method does not record the full effect of benzedrine when administered in sufficiently large amounts to produce considerable localized movements of the head region. There is evidence that, within a small dosage range, an increase in the drug produces an increase in the level of activity. For a group of rats the 3 mg./kg. dose appears to produce a greater increase in activity than either the $1\frac{1}{2}$ mg. or $4\frac{1}{2}$ mg. doses, although for individual rats the optimal dose varies.

2. Relation of Effect of Dose to Resting Level of Activity

In the foregoing analysis the effect of the injection of benzedrine was expressed as an increase over the resting level of activity. The question arises as to whether or not the drug's effect is conditioned upon this factor of the rat's general level of activity. On the basis of the average activity shown on the rest days the six most active rats were placed in one group and the six least active rats in another. The percent increase in activity for the three doses for these extreme groups is found in Table IV. Comparing the two

groups in terms of the effect of the three doses reveals the fact that in all three instances the least active rats are stimulated to a far greater relative level of activity than are the most active animals. On the average, benzedrine increases the

TABLE IV

PERCENT INCREASE IN ACTIVITY ABOVE THE RESTING LEVEL FOR THE SIX MOST ACTIVE AND THE SIX LEAST ACTIVE RATS FOR THE THREE DOSES

Most Active				Least Active			
Number of Rat	1½ mg./kg.	3 mg./kg.	4½ mg./kg.	Number of Rat	1½ mg./kg.	3 mg./kg.	4½ mg./kg.
12	334	421	288	11	226	123	68
2	273	216	168	4	87	167	106
3	196	268	239	7	67	145	171
5	188	214	238	1	77	143	158
15	211	180	149	14	63	106	111
9	217	343	327	10	185	161	117
Average	237	274	235		121	141	122

activity of the least active rats about 2½ times, whereas it increases the activity of the most active rats only about 1¼ times. Furthermore, the relative potency of the 3 doses is the same for the two groups. If the percent increase in activity of the least active animals is expressed in terms of the percent increase of the most active, the following figures are obtained: 1½ mg. 1.96, 3 mg. 1.95 and 4½ mg. 1.93. These data mean that for each of the three doses the least active animals are stimulated to run 1.9 + times as much as the most active. For both groups the 3 mg./kg. dose shows the greatest increase, the two other doses being about equal in effectiveness. Apparently the effect of benzedrine on activity, as reflected in the activity cage method, varies with the general activity level of the rat, producing a greater percent increase in the less active animals. However, the relative effectiveness of the three doses is the same for inactive rats as it is for the very active animals.

3. *Variation in Activity during the 4-hour Experimental Period*

In the previous study (1), it was shown that the maximal effects of the drug occurred in the first hour after the injections

and that after about 4½ hours most of the effect of the benzedrine had disappeared. A better notion of the temporal variation of the effects of the drug was obtainable in the present study because records of the activity were taken every fifteen minutes instead of every hour as in the first experiment. Table V presents the activity records, by

TABLE V

RECORD OF ACTIVITY BY QUARTER-HOUR INTERVALS FOR THE EXPERIMENTAL PERIOD FROM 9 A.M. TO 1 P.M. FOR EACH OF THE THREE DOSES

Time	Average Number of Revolutions per Rat		
	1½ mg./kg.	3 mg./kg.	4½ mg./kg.
9:15	204	230	220
9:30	112	100	72
9:45	86	71	44
10:00	60	71	41
10:15	46	62	33
10:30	57	66	42
10:45	56	47	44
11:00	50	61	52
11:15	35	49	52
11:30	31	51	43
11:45	28	33	46
12:00	24	18	44
12:15	20	12	33
12:30	12	14	48
12:45	11	14	25
1:00	10	14	21

quarter-hour intervals, for the experimental period from 9 A.M. to 1 P.M. It will be seen that for all three doses the greatest effect occurs in the first fifteen minute period. The second period is also relatively high for the 1½ mg. and the 3 mg. doses. From this point on there is a rather constant decrease in the effect of these two doses. The differences between the 1½ mg. and the 3 mg. doses are not great, but there is a slight tendency for the 3 mg. dose to maintain a higher level of activity throughout most of the period.

Examination of the record for the 4½ mg. dose immediately reveals the effect of the excessive head sway and rooting movements on the activity record. Immediately after the first fifteen minute period the activity from this dose is considerably less than that from either of the other doses.

This lowered activity level lasts for about 1½ hours, after which time the head and rooting movements begin to disappear and the level of activity increases. For the last 1½ hours of the 4-hour period the 4½ mg. dose has a considerably greater effect than either the 1½ mg. or the 3 mg.

4. Relation of Weight and the Effect of Benzedrine

The effect of a large number of drugs is conditioned upon the weight of the animal. To determine if weight were a factor in the effectiveness of benzedrine the rank order correlations were computed between the average weight of the animals during the experimental period and the average activity on drug days, using the activity of the 4-hour experimental period. Coefficients between weight and number of revolutions, for the three doses, are as follows: 1½ mg. + .21, 3 mg. + .15 and 4½ mg. + .19. When the percent increase in activity over the resting level is used the correlations are: 1½ mg. − .01, 3 mg. + .02 and 4½ mg. − .08. When the rat's maximal percent increase over his resting level, regardless of the dose causing it, is correlated with his weight the rank order coefficient is + .05. It is apparent from these figures that the weight of the animal is not an important factor in determining the effectiveness of benzedrine as studied by the revolving cage method.

Conclusions

The following statements seem warranted from the findings of the present experiment:

1. A 3 mg./kg. dose of benzedrine sulfate produces a greater increase in activity than either a 1½ mg./kg. or a 4½ mg./kg. dose.

2. The optimal dose of benzedrine varies with different animals.

3. When the effect of the benzedrine is computed as a percent increase in activity over the resting activity, the 3 mg./kg. dose is more effective than either the 1½ mg./kg. or the 4½ mg./kg. dose.

4. The effect of benzedrine varies with the general resting

activity level of the rat, being greater for the less active animals.

5. All three doses produce a greater relative increase in the less active rats than in the more active, the effect being about the same for all doses.

6. The increase in activity resulting from benzedrine is greatest for the first 15 minute period and almost wholly disappears after about 4½ hours.

7. No relation was found to exist between the average weight of the animals and the effect of benzedrine in increasing their activity.

Summary

The effect of variation in the dosage of subcutaneous injections of benzedrine sulfate was measured in 15 white rats by the revolving activity cage method. Doses of 1½ mg./kg., 3 mg./kg. and 4½ mg./kg. were studied. All rats received each dose twice. The 3 mg./kg. dose produced the greatest increase in activity in terms of both absolute number of revolutions of the cages and the percent increase in activity above the resting activity level. Benzedrine has a greater effect on inactive rats than upon the very active animals. The effect of the benzedrine is not conditioned upon the weight of the animal.

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Bibliography

1. Searle, L. V., and Brown, C. W., The effect of subcutaneous injections of benzedrine sulfate on the activity of white rats, *J. Exper. Psychol.*, 1938, **22**, 480–490.

STUDIES IN THERMAL SENSITIVITY: 6. THE REACTIONS OF UNTRAINED SUBJECTS TO SIMULTANEOUS WARM + COLD + ELECTRIC SHOCK

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In a previous article,[1] the reactions of untrained subjects to simultaneous warm + cold stimulation were described. Unlike the traditional results with trained observers, these untrained individuals did not report 'hot' from compound grill stimulation, except in a few scattering instances. In fact, the intensity of the warmth seemed to be diminished rather than accentuated by the addition of cold. To account for the discrepancy, it was suggested that the common 'hot' may be a different experience from the trained observer's 'heat'; and that the Alrutz theory may apply to the latter, but not to the former.

The synthesis of 'burning heat' from warm + cold + pain has also been cited as evidence for the Alrutz theory. Ferrall and Dallenbach [2] secured the addition of pain to the warm-cold complex by using a low-voltage alternating current wired to alternate tubes of the grill. The temperature of the cold system was varied from 9° to 20° C., and of the warm system from 38° to 42° C. (for observer *J*) and from 40° to 44° C. (observers *A* and *D*), to keep below the heat limen previously determined for each individual. The electric current was varied between 10 and 15 volts at random. Control tests showed that 'burning heat' was not reported from the electric shock alone when the grill systems were at skin neutral. With

[1] Jenkins, W. L., Studies in thermal sensitivity: 5. The reactions of untrained subjects to simultaneous warm + cold stimulation, *J. Exper. Psychol.*, 1938, **22**, 451–461.

[2] Ferrall, S. C. and Dallenbach, K. M., The analysis and synthesis of burning heat, *Amer. J. Psychol.*, 1930, **42**, 72–82.

warm + cold + shock, however, burning heat was reported by the three observers as follows:

D (trained in introspection and experienced in grill experiments)—in 34 out of 40 trials.
J (trained in introspection but with no previous experience in cutaneous experiments)—in 12 out of 36 trials.
A ('totally untrained in introspection')—in 6 out of 48 trials.[3]

Concerning the discrepancy, the authors say: "The difference between the results of *D*, on one hand, and *A* and *J*, on the other, may be due to:

(1) 'set'; *D* knew the object and plan of the experiment, and he may unconsciously have been biased in his reports in the direction of burning heat (*D* admits this possibility, but he is certain that he had not anticipated a particular result, and that the qualities he reported were matters of indifference to him);
(2) difference in experience and training. *D* was highly trained in cutaneous observation in general and in the introspection of heat in particular; *J*, though an experienced *O*, had never before observed in a cutaneous experiment; and *A* was a novice;
(3) differences in the effectiveness of the stimuli—particularly the electrical stimuli. The cutaneous stimuli were, as the experiments of Series V indicate, more effective for *D* than for either *A* or *J*, possibly because of differences in the amount of pressure of the arm upon the grill or in the amount of perspiration which bathed the arm." [4]

Some years earlier, Knight had made a study of the integration of punctiform warm + pain.[5] Mapped 'warm spots' were stimulated by radiant means until a moderate experience of warmth ensued. Then the skin nearby was lightly pricked

[3] *Op. cit.*, 79–80.
[4] *Op. cit.*, 80.
[5] Knight, L., The integration of warmth and pain, *Amer. J. Psychol.*, 1922, **33**, 587–590.

with a needle. Three introspectively-trained observers were used. The results are not reported quantitatively, but the first sentence of her conclusions is most suggestive: "The simultaneous stimulation of warm and pain spots may result in an experience which is variously called 'burning heat,' 'burn' and 'hotness.'" [6]

The significance of these findings seems to have been generally overlooked. Taken literally, they mean that burning heat can be synthesized from warm + pain, with no cold present. If we take 'hotness' to be the equivalent of 'heat,' then heat itself can be experienced with just warm + pain. In either case, the implication is that cold is not a necessary condition for synthetic heat.

Unfortunately, the integration of warm + pain in the grill technique does not seem to have been tried with trained observers. Since only untrained subjects were at our disposal, we determined to attempt the experiment with them. If any positive effects were obtained, it would be possible to compare the results from warm + cold + pain with those from warm + pain. If the original simple stimulation were reported as 'hot,' it would be possible to see if this could be changed to 'burning hot' by the addition of simple electric shock.

Apparatus and Procedure

Grill No. 1 from the warm + cold experiments was used throughout. In this grill, the cold bars can be dropped about 5 mm below the surface and raised into position by a cam action. Warm and cold bars are 3 mm wide, with approximately a 1 mm air space. Alternate bars were wired to the two leads from a transformer connected to the lighting circuit. The current used was approximately 9 volts.

Since an important variable in shocking is the condition of the skin, the surface of the grill was moistened with a strong salt solution before each stimulation. In spite of this precaution, it soon became evident that the effectiveness of the shock was not the same for all subjects. Some experienced a sharp pain; others only a mild tingling; while in a few cases there was apparently no effect at all.[7] Control tests give some idea of the general effectiveness of the shock. With both temperature systems at neutral, the shock alone produced some non-thermal report (tingle, sting, ache, pain, etc.) in 83 percent of the subjects. About 10 percent of them, moreover, reported the simple shock as 'hot' or 'burning hot.'

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[6] *Op. cit.*, 590.

[7] Ideally, the strength of the current should be adapted for each individual. Practically, it was impossible to do this without disclosing the technique of the experiment to the subjects.

The procedure was the same with all subjects. The simple warm stimulation was given first. After five seconds, the movable bars were brought up in contact with the skin and the shocking current turned on. After five seconds more, the subject removed his arm and went out to write his report. Subjects were handled in groups of about ten, and with a few exceptions had also served in the earlier experiments upon warm + cold stimulation. They were not informed of the plan or purpose of the experiments, which most of them seemed to assume were merely a continuation of the earlier work.

All reports were made on the same form, which had separate columns for Experience *A* (first five seconds) and Experience *B* (second five seconds). The formal instructions were:

"In the columns headed 'Therm.' report all thermal (temperature) sensations in each experience, using the following symbols:

V. C.—very cold	O—neutral	V. W.—very warm
COLD—cold	S. W.—slightly warm	HOT—hot
COOL—cool	WARM—warm	BURN—burning hot

In the columns headed 'Non-therm.' report all non-thermal experiences, particularly pain. Try to characterize each experience with a single word if possible, such as 'tingle,' 'prick,' 'sting,' 'ache,' etc."

POSSIBLE ARTIFACTS

In addition to the possibility that electric shock alone may be reported rather rarely as 'hot' or 'burning hot,' there are two possible sources of artifacts in the experiments as conducted.

1. All temperatures were measured at the *inlet* and are not necessarily the temperatures of the grill surfaces themselves. Calibration tests showed that the outlet temperature of the warm water was not appreciably affected by the temperature of the water which flowed through the cold bars; whether these were in the raised or dropped position, the inlet-outlet differential remained about 2° C. But this is no guarantee that the surface temperatures were not altered. Since multiple thermocouple-galvanometer measurements of the surface temperatures were not feasible, a physiological check was substituted. If the surface temperature of the warm bars was materially affected by the temperature of the intervening cold bars, this should show in a reduced percentage of 'hot' and 'burning hot' reports with simple stimulation (*i.e.*, with the cold bars in the dropped position). Table 1 gives the results of this

TABLE 1

PERCENTAGE OF HOT AND BURNING HOT REPORTS WITH COLD BARS IN DROPPED POSITION

Cold Bars at	Warm Inlet Temperatures					
	34°	40°	42°	44°	46°	48°
34°	0.0	5.1	10.6	19.6	59.9	84.2
25°	—	1.2	10.8	21.5	48.5	—
20°	—	5.6	7.4	32.6	54.7	—
15°	—	5.3	10.6	22.9	39.5	—
10°	—	3.3	5.0	20.0	17.5	—

check. With cold temperatures of 25° and 20° C., no effect is evidenced. With cold temperatures of 15° and 10° C., there may be some, especially with the higher levels of warmth. This should be taken into account in interpreting the results of 10° and 15° C. compound stimulation.

2. Using the same *order* of stimulation throughout may have introduced another artifact. Even without shock, when the arm is in continuous contact with the grill, the experience may increase in intensity so that the report for the second five seconds might naturally be higher. On the other hand, adaptation might occur so quickly that the intensity of the second report would naturally be lower. A partial check is available from the effects of 48° and 50° C. stimulation without shock (see Table 2), designed

TABLE 2

COMPARISON OF EXPERIENCES *A* AND *B* WITHOUT SHOCK

Change	48°	50°
Same report	51.6%	59.2%
Warm to hot	16.5	7.4
Warm to burning hot	2.2	12.4
Hot to burning hot	16.5	14.8
o to warm or hot	0.0	2.5
Decreases (hot or burning hot to warm)	13.2	3.7

primarily to show that subjects would report 'hot' and 'burning hot' with simple stimulation at high temperatures.[8] At 48° C., the *A* and *B* reports were the same in somewhat more than half the cases, but with the balance, there is a definite preponderance of increases over decreases. It is regretted that data are not available for the lower temperatures.

RESULTS

The figures presented in Table 3 are percentages of 'clear chances.' There was a 'clear chance' for a change from warm → hot or warm → burning hot, only when the original report was not 'hot' or 'burning hot.' Similarly, there was a 'clear chance' for a change from hot → burning hot only when the original report with simple stimulation was 'hot.'

1. With the movable bars at 34° C., the addition of *shock* results rather uniformly in an increase in the intensity of the experience reported. An original report of 'warm' is changed to 'hot' or 'burning hot' in a significant percentage of cases with inlet temperatures of 42° C. and above. How much of this is due to the artifacts previously noted cannot be precisely determined. A direct check is possible only in the case of 48° C. stimulation. Here an original report of warm was changed to hot or burning hot in 84.7 percent of the cases.

[8] With 48° C. without shock, hot or burning hot was reported 85.8 percent of the time; with 50° C. 96.7 percent.

Subtracting 10.2 percent for the effect of shock alone,[9] and 18.7 percent for the natural increase,[10] there still remains a percentage of 55.8 percent which appears to be directly the

TABLE 3

PERCENTAGES OF CLEAR CHANCES

(*A*) *Warm → Hot*

With	34°	40°	42°	44°	46°	48°
34°	5.6	9.2	18.9	23.2	31.0	46.2
25°	—	10.8	13.3	18.2	18.7	—
20°	—	2.9	8.0	5.0	16.7	—
15°	—	1.9	3.9	8.1	4.3	—
10°	—	3.5	5.3	12.5	6.1	—

(*B*) *Warm → Burning Hot*

With	34°	40°	42°	44°	46°	48°
34°	4.6	9.2	13.5	18.9	12.7	38.5
25°	—	9.7	7.2	7.6	6.3	—
20°	—	2.0	4.0	1.7	0.0	—
15°	—	1.9	2.0	5.4	8.7	—
10°	—	3.4	0.0	0.0	6.1	—

(*C*) *Warm → Hot or Burning Hot*

With	34°	40°	42°	44°	46°	48°
34°	10.2	18.4	32.4	42.1	43.7	84.7
25°	—	20.5	20.5	25.8	25.0	—
20°	—	4.9	12.0	6.7	16.7	—
15°	—	3.7	5.9	13.5	13.0	—
10°	—	6.9	5.3	12.5	12.2	—

(*D*) *Hot → Burning Hot*

With	34°	40°	42°	44°	46°	48°
34°	—	57.2	40.0	42.5	42.5	79.1
25°	—	0.0	0.0	6.7	23.1	—
20°	—	0.0	0.0	0.0	0.0	—
15°	—	0.0	0.0	0.0	8.3	—
10°	—	0.0	0.0	0.0	14.3	—

result of the synthesis of warm + pain. As might be expected, burning hot can also be derived from hot + pain.

2. Throughout all the tables, the addition of cold to the complex tends to mask rather than to accentuate the synthetic

[9] See 34° C. + 34° C. + shock, in Table 3-C.

[10] See Table 2.

effects. Except with 25° C., the results are negligible. The only known artifact here is the effect of the cold bars upon the surface temperature of the warm bars. In Table 1, it was shown that the reports of 'hot' with simple stimulation are not significantly fewer when the cold bars are at 25° or 20° C. instead of 34° C. Yet clearly the compound effects are much lower with 25° C. and almost disappear at 20° C.

TABLE 4

SUBJECTS REPORTING HOT AND BURNING HOT

	With Simple Stimulation plus Shock	With Compound Stimulation plus Shock
Warm → hot	65	28
Warm → burning hot	44	23
Warm → hot or burning hot	74	37
More than 50 percent of clear chances	35	8
Hot → burning hot	49	6

Table 4 presents the situation from another angle. 74 out of 108 subjects reported the change from warm to hot or to burning hot at least once with simple stimulation plus shock. 35 of these gave such reports in more than half of the clear chances; 8 of them in 100 percent of the cases. In contrast, only 37 subjects reported changes from warm to hot or burning hot with compound stimulation plus shock, and only 8 of them in more than half of the clear chances. Significantly, 7 of these 8 subjects gave equal or better records with simple stimulation plus shock.

DISCUSSION OF RESULTS

With untrained subjects, it is now evident that reports of 'hot' and 'burning hot' can be secured from a variety of stimulus situations.

Reports of 'hot' can be obtained:

(*a*) Frequently with warm + shock.
(*b*) Occasionally from warm + cold.
(*c*) Rarely from neutral + shock.

Reports of 'burning hot' can be obtained:

(*a*) Frequently with hot + shock.

(*b*) Less frequently with warm + shock.
(*c*) Occasionally with warm + cold + shock.
(*d*) Rarely with neutral + shock.

This gives us a new light on the synthetic approach with untrained subjects. If 'hot' and 'burning hot' can be obtained from so many stimulus combinations, what is the value of any *one* of them. What point would there be, for example, in attempting to develop a new technique of compounding warm and cold which might give positive results? Evidently cold is not a *necessary* condition for the synthetic experience of 'hot,' because it can be obtained in abundance without cold.

Does this prove that the Alrutz theory (as applied to the common experience of 'hot') is wrong? Not at all! The negative results with warm + cold, coupled with the positive results from warm + shock, may well make us view the theory with some suspicion. Clearly, the synthetic 'hot' occurs more readily in the absence of cold . . . but nevertheless cold receptor stimulation may be a necessary part of the *normal* experience of 'hot.' Only physiological analysis can give us the answer.

The positive results with warm + shock make us curious as to what might happen with trained observers under similar conditions. Can the trained observers' 'heat' be synthesized from the warm + shock, as well as from warm + cold? This is a question which only further research can answer.

Summary

108 untrained subjects were tested with a variety of compound temperature stimulations plus mild electric shock. With simple warm stimulation plus shock, clear reports of 'hot' and 'burning hot' were obtained. When cold was added to the complex, the positive effects fell off sharply and were virtually negligible at the lower temperatures. It has now been shown that 'hot' can be synthesized quite readily from warm + shock, infrequently from warm + cold, and occasionally from neutral + shock. It is therefore main-

tained that the synthetic approach can give us no light upon the normal physiological basis for the common experience 'hot.' Because of the largely negative results with warm + cold, and the positive results with warm + shock without cold, it is suspected that the Alrutz theory (simultaneous stimulation of warm and cold receptors) does not apply to the common experience of 'hot.' But the final test must be analysis, not synthesis. It is suggested that similar experiments with introspectively-trained observers would be desirable.

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RETROACTIVE INHIBITION: THE INFLUENCE OF DEGREE OF ASSOCIATIVE VALUE OF ORIGINAL AND INTERPOLATED LISTS

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Recent studies have amassed an imposing array of evidence pointing toward a transfer theory of retroactive inhibition.[1] Most of this evidence has been based on the fact that retroaction varies directly with similarity of the original and interpolated materials and conditions. This similarity between the two activities may be in any one or more of several dimensions. The original and interpolated activities may involve the same operation, *e.g.* learning. They may involve the same content, such as adjectives, nonsense syllables, or chess formations. They may be similar in meaning, temporal and spatial continguity, degree of organization, environmental conditions, or any of a number of other factors.

The problem of the experiment reported here was to determine the effect of varying the degree of associative value of the original and interpolated lists. If, as is assumed by the transfer theory, retroaction occurs because of inter-list confusion or blocking, this inter-list effect should be diminished if the two lists are widely different in the ease with which they form associations. This differs from the factor of similarity in meaning in that in the case of meaning, the similarity is usually apperceived by the subject.[2] In the

[1] For summaries of the literature on retroactive inhibition, the reader is referred to Britt, S. H. Retroactive inhibition: A review of the literature. *Psychol. Bull.*, 1935, **32**, 381–440. McGeoch, J. A. Learning and retention of verbal material. *Psychol. Bull.*, 1934, **31**, 381–407. The major theories of retroactive inhibition are discussed by Britt, S. H. Theories of retroactive inhibition. *Psych. Rev.*, 1936, **43**, 207–216.

[2] There is no real evidence for this statement, so far as the author knows. McGeoch and McGeoch found no difference in amount of retroaction with or without recognition of synonymity. (Studies in retroactive inhibition: VI, The influence of the relative serial position of interpolated synonyms. *J. Exper. Psychol.*, 1936, **19**, 1–23.) They

present case it is supposed that inter-list association and confusion will be enhanced when both lists are composed of items of the same associative value, while each will be more completely insulated from the other if they differ widely in this respect.

Procedure.—The materials employed in this experiment [3] were nonsense syllables chosen from Glaze's [4] classification according to associative value. Eight lists of eleven syllables each were selected, four being composed of syllables of 100 percent associative value and the other four of 0 percent associative value. These lists were presented on a motor-driven memory drum at a 2-second rate and were learned by the anticipation method. In this method, the first word serves as a stimulus or cue word and is not (necessarily) learned. The subject, then, must learn a list of 10 items. The syllables were spelled out rather than pronounced.

In order to control to some extent the degree of learning of the different lists where associative value varies so radically, a change in the usual conditions was adopted. Rather than presenting the lists for a given number of trials in original and interpolated learning, the lists were learned to a criterion, which was six correct anticipations in one trial. In other words, learning was continued with each list until the subject was able to anticipate correctly 6 of the 10 words.[5] In this way, more trials were taken on the lists of 0 percent associative value, and this may have involved some overlearning of some of the items. This possibility should be borne in mind.

Four conditions were studied,[6] as follows: (1) both original and interpolated lists of 100 percent associative value, (2) both lists of 0 percent associative value, (3) the original list of 100 percent and the interpolated list of 0 percent associative value, and (4) the original list of 0 percent and the interpolated list of 100 percent associative value. Sixteen subjects went through all of the four conditions in a counterbalanced order, and the specific lists were rotated among the different conditions to avoid the effects of

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had insufficient data, however, on which to base any conclusion. It seems reasonable to suppose that retroaction will be found to vary directly with awareness of similarity between the two activities.

[3] The experimental work was done by Mr. Conrad Keiner under the author's supervision.

[4] Glaze, J. A. Classification of nonsense syllables. *J. Genet. Psychol.*, 1928, **35**, 255–269. Syllables which suggested some meaning to all of 15 subjects when tachistoscopically exposed were classified as 100 percent. Those suggesting meaning to none of the 15 subjects were rated as of 0 percent associative value.

[5] This would not, of course, be always exactly six, since a subject might jump from five to seven or even eight correct anticipations on adjacent trails. That it was close enough in terms of averages is indicated by the following figures: for the four conditions of original learning the mean numbers of items correctly anticipated on the last trial were 6.56, 6.25, 6.37, and 6.37; for the four interpolated conditions, 6.41, 6.68, 6.37, and 6.87. None of the differences are systematic in direction nor significant in terms of standard errors.

[6] Since it was not intended to show that retroactive inhibition occurs under these conditions, but only to compare the amounts occurring under the different conditions, no rest condition was included. Comparisons, then, will be made between the different work conditions.

particular difficulty of any one list. Each experimental session was conducted according to the same scheme. An original list was learned to the criterion of six correct anticipations. After a pause of 30 seconds, during which a new list was prepared on the drum, an interpolated list was learned to the same criterion of six correct anticipations. Ten minutes after the last trial of original learning, relearning began and continued to three successive errorless trials. The interval between interpolated learning and relearning was occupied by the reading of humorous material.

Results.—The mean numbers of trials required to reach the criterion of six correct anticipations in original and interpolated learning are given in Table I. As might be expected, this figure varies with the associative value of the lists. The four values for the 100 percent lists are in fairly close agreement (5.4, 6.4, 6.5, 5.8), as are the values for the 0 percent lists with one exception. It would appear that there is a positive transfer effect in condition *B* from original to interpolated lists, rather than the usual negative transfer associated with inter-list confusion.[7] This is to be expected with lists of low associative value, where the cross associations between lists would be weak, and so positive transfer could be mediated through similarity in method (practice effect).

TABLE I

MEAN NUMBERS OF TRIALS FOR LEARNING ORIGINAL AND INTERPOLATED LISTS

Condition	Original		Interpolated	
	Mean	σ_m	Mean	σ_m
A	5.37	.84	6.44	.51
B	13.81	2.22	10.12	.79
C	6.50	.64	12.87	1.97
D	12.06	1.12	5.81	.40

The same trends are revealed in the figures for the mean numbers of correct anticipations on original and interpolated lists given in the second table.

Retention is measured in four ways:[8] (1) Recall 1, which is

[7] This negative transfer effect may be slightly evidenced in condition *A*, but this is doubtful, since even though the value for interpolated learning is higher than its corresponding original learning, it is lower than the value for original learning under condition *C*.

[8] These measures are used by McGeoch, J. A., and McDonald, W. T. Meaningful relation and retroactive inhibition. *Amer. J. Psychol.*, 1931, **43**, 579–588; and by McGeoch in all subsequent work on retroaction (*cf.* references in Britt, *op. cit.*).

TABLE II

MEAN NUMBER OF CORRECT ANTICIPATIONS ON ORIGINAL AND INTERPOLATED LISTS

Condition	Original		Interpolated	
	Mean	σ_m	Mean	σ_m
A	18.06	2.50	18.62	1.89
B	31.68	3.81	26.94	2.13
C	20.25	1.89	33.19	4.59
D	33.75	3.33	19.12	1.54

the number of correct anticipations on the first relearning trial; (2) Recall 2, which is the number of correct anticipations on the second relearning trial; (3) Relearning 1, which is the number of trials required to learn to (but not including) one errorless trial; and (4) Relearning 2, which is the number of trials required to relearn to (but not including) three successive errorless trials. The mean recall and relearning scores for the four conditions of this experiment, with their standard errors, are listed in Table III.

TABLE III

MEAN RECALL AND RELEARNING SCORES

Condition[9]	Recall 1		Recall 2		Relearning 1		Relearning 2	
	Mean	σ_m	Mean	σ_m	Mean	σ_m	Mean	σ_m
A (100, 100)	.88	.22	3.56	.16	8.69	1.05	13.38	1.89
B (0, 0)	1.25	.40	3.25	.38	19.69	1.60	28.44	3.22
C (100, 0)	1.38	.38	4.31	.73	9.50	1.33	14.25	2.24
D (0, 100)	2.44	.65	3.69	.38	14.69	1.33	23.38	2.22

It is at once apparent that no general trend is evidenced by these figures. The fact that learning continued for different numbers of trials, makes some relative scoring method necessary. Such scores have been figured for each of the four measures of retention.[10] For recall scores, this was done by means of the formula:

[9] The figures in the parentheses indicate the associative values of the original and interpolated lists under each condition.

[10] These scores were computed from both individual and mean values, except in the case of recall 1 where individual savings scores cannot be computed because of the fact

$$100 \times \frac{\text{correct anticipations on recall trials}}{\text{correct anticipations on last trial of learning}}.$$

For relearning scores, the formula below was employed:

$$100 - \left(\frac{\text{no. of trials to relearn to criterion}}{\text{(no. of original learning trials}} \times 100\right).$$

These values are presented in the table below:

TABLE IV[11]

RELATIVE SCORES FOR RECALL AND RELEARNING

Condition	Recall 1	Recall 2	Relearning 1	Relearning 2
A	13.41	55.14	−73.26	−176.76
B	20.00	52.12	−69.02	−174.86
C	21.67	64.55	−56.29	−130.41
D	38.30	57.31	−35.57	−116.19

The saving scores for Relearning 1 and 2 are negative, indicating, of course, a loss rather than a saving. In the case of each score, the *C* and *D* conditions show less loss (or more saving) than the *A* and *B* conditions. Conditions *A* and *C* each involve the learning and relearning of a list of 100 percent associative value, but with different interpolations. In every case, condition *C* shows less loss as a result of the interpolation than condition *A*.[12] The same comparison can be made between conditions *B* and *D* which differ only in the nature of the interpolation. Where the interpolated list is similar to the original in associative value, the saving is less than that obtained when the two lists differ in this respect. This effect is most noticeable in terms of Relearning scores where there is a fairly well-marked break between the *A*, *B* and *C*, *D* conditions.

that several subjects have zero for the recall 1 score which would enter into the dividend of the formula. The trends are, of course, the same.

[11] The values for recall 1 are computed from the averages; the remaining values are the means of the individual savings scores (*cf.* footnote 10).

[12] Of course it cannot be proved in the absence of a rest condition that the loss in any case occurs because of the interpolated condition. However, the whole mass of previous work on retroaction is witness to this statement. In any event, the *difference* in loss between *A* and *C*, and between *B* and *D*, does occur because of the interpolated condition, since that is the only factor in which the two conditions vary.

There is also, except in the case of Recall 2 scores, a regular tendency for the amount of retroaction to decrease from condition *A* to condition *D*. Condition *A*, where both lists are of high associative value, might be expected to yield most retroaction, by a transfer theory, since there would be in this case more cross associations between lists and consequently more inter-list confusion than in condition *B*, where both lists are low in associative value. It is not entirely clear, however, why condition *D* should yield less retroaction than condition *C*. The answer may lie in the fact of greater overlearning of some of the items in the original list in condition *D*. This is hardly possible, however, since it is not revealed in the overlearning ratios. In fact, these overlearning ratios are uniformly higher for the 100 percent lists than for the 0 percent lists.[13]

Discussion.—By associative value, as used above, is meant the ease or difficulty with which items enter into association. This association commonly occurs, of course, through meanings. With nonsense syllables, as in the present instance these meanings are inferred by the subjects. Therefore, nonsense syllables of 100 percent associative value are items which are readily associated with each other through these implied meanings. These associations may be either with other items of the same list, especially when learning is by the anticipation method, or with other syllables experienced in the same way and at about the same time. Such is the case with an interpolated list as in this experiment. These inter-list associations, between original and interpolated lists, would cause confusion at the relearning of either list, or retroactive inhibition.

Insofar, then, as condition *A* in this study yields more retroaction than condition *B*, the results may be explained by reference to transfer through similarity in meaning, since the inter-list associations would be much more prevalent in *A*

[13] Overlearning ratios were computed from average data by the formula:

$$100 \times \frac{\text{correct anticipations}}{\text{failures to anticipate}}.$$

The values are: for original learning, 50.25, 29.80, 45.25, 38.68; for interpolated learning, 43.93, 36.37, 34.64, 49.18 for conditions *A*, *B*, *C*, *D*, in that order.

than in *B* where the items are relatively devoid of meaningfulness. Something more seems necessary to explain the even lesser extent of retroaction in conditions *C* and *D*. By transfer through meaning alone, condition *B* might be expected to yield the least amount of retroaction since both lists are of 0 percent associative value.

We suggest, as a tentative hypothesis, that where the two activities are mutually isolated and insulated in the total organization of behavior, by whatever means this isolation can be achieved, retroaction will be reduced to a minimum. In the present case, varying the degree of associative value of the two lists increases this separation. Both lists would tend to be consolidated as separate constellations of associations in the whole mental organization, since they differ so radically in meaningfulness and probably in means of learning. In the 0 percent lists, where meanings are inferred with such difficulty, learning would be practically rote, and associations factitious and arbitrary. Such a list would be more coherent. It would not tend to be confused with a list whose items readily admit of meaningful associations, but might be confused to some extent with other items of the same kind.

This hypothesis of retroaction would put the locus of the greater part of the transfer effect at the time of recall. The subject, at recall, searches his memory for the proper item, and either gets a wrong one which is similar in form or meaning to the correct one,[14] or is blocked because of such confusion and makes no response in the time allowed. If the mental organization representing the memorial substrates of the two activities is a total inter-associated mass, retroaction will be maximal. Anything which serves to distinguish one constellation of associations from another will facilitate the desired recall, and thus reduce retroaction.

Summary.—Sixteen subjects went through four experimental conditions in a counterbalanced practice order. These conditions were (1) learning and recall of a list of nonsense syllables of 100 percent associative value, with interpolation of a second list of the same kind; (2) learning and recall of a

[14] McKinney, Fred, and McGeoch, J. A. The character and extent of transfer in retroactive inhibition: disparate serial lists. *Amer. J. Psychol.*, 1935, **47**, 409–423.

list of 0 percent associative value with interpolation of a second list of the same kind; (3) learning and recall of a 100 percent list with interpolation of a 0 percent list; and (4) learning and recall of a 0 percent list with interpolation of a 100 percent list.

1. In terms of both recall and relearning, the second condition, with both lists of 0 percent associative value, resulted in less retroaction than the first condition.

2. In terms of both recall and relearning, interpolating a list of different associative value resulted in less retroaction than interpolating a list of the same associative value.

3. A possible explanation is suggested in terms of isolation of the memorial substrates of the two lists.

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THE HYPNOTIC INDUCTION OF HALLUCINATORY COLOR VISION FOLLOWED BY PSEUDO NEGATIVE AFTER-IMAGES

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Previous clinical experimentation on the induction by hypnosis of various hallucinatory phenomena suggested the possibility of inducing hallucinatory color vision. Accordingly, an experiment was devised involving the hallucination of the colors red, green, yellow and blue in response to direct hypnotic suggestion, followed by the spontaneous hallucination by the subjects of the complement of each of these colors in a fashion comparable to the development of negative after-images.

The experiment was conducted on 5 university freshmen, selected from among commerce and engineering students, all of whom had been used repeatedly for hypnotic work but not in connection with the hallucination of colors. In addition, they had all been trained over a long period of time to develop exceedingly deep hypnotic traces, and for this experiment, from 30 to 45 minutes of continuous suggestion was given after a trance had been induced to insure a consistently profound hypnotic trace.

Procedure

The procedure employed was comprised of the following steps:

1. The administration in the waking state of a word association test containing a hundred words, among which were 'red,' 'green,' 'yellow,' 'blue' and 'bright.'

2. The induction of a profound somnambulistic trance and the administration of a second word association test, also containing a hundred words, some of which were common to the first list, and among which in a different order were the same critical words.

3. The giving of the following instructions while the subject continued in the deep hypnotic trance:

"I am going to show you, one at a time, sheets of colored paper. Each sheet is entirely of one solid color, and this color will be very bright. All of the sheets will be

colored, but no two successive sheets will be of the same color. I will name the color of the first sheet, and you will name the color of the next sheet, and we will continue in this alternate fashion. As I name the color of the first sheet, you will look at it carefully until you see it plainly and clearly, just exactly as I have described it. Nod your head as soon as you see it plainly, and I will then put it out of sight in the left hand drawer of the desk and take a new one from the right hand drawer. As I show you this next sheet, you are to look at it carefully and tell me what color it is. It, too, will be of a single bright color, but a color different from the one I named. After you have named the color of your sheet, it will then be my turn to name the color of the next sheet, then your turn will come." These instructions were repeated several times to insure full comprehension.

4. The exhibition, one at a time, of 16 sheets of plain white typing paper. Of these, the 8 alternate sheets were described by the experimenter in the following order as, 'red,' 'yellow,' 'green,' 'blue,' 'green,' 'yellow,' 'red' and 'blue,' the adjective 'bright' being added each time, while the subject was called upon to identify the 8 intervening sheets as of some one bright color other than that just named by the experimenter.

5. The exhibition, as a control measure against the development of strong associative values between the complementary colors, of brightly hued red, yellow, blue and green sheets of paper. These sheets were presented in pairs, with each color paired with itself and with each of the other three colors, making a total of 16 pairings exhibited in random order. As these pairs were presented, the subject named the colors aloud.

6. The repetition of Step 4. followed by questioning of the subjects concerning the color intensity of the various sheets. Such questioning had been avoided previously because of the possible effect of suggestion.

7. The administration of the first word association test while the subject was still in the trance state.

8. The administration of the second word association test after awakening the subject.

9. Questioning concerning the definition of complementary colors, and the hazarding of guesses by the subjects when the definition of complementary colors had been given by the experimenter.

Results

The results obtained from this experimental procedure may be listed as follows:

1. The preliminary word association test disclosed no direct association of the various colors with their complements. Rather, the subjects' associations for red and blue were heavily weighted by academic associations, such as ink, drawing, debits, and bluebook.

2. The exhibition for about 1 to 2 minutes each of the 8 alternate sheets of white paper disclosed 4 of the 5 subjects as being fully capable of 'hallucinating' in every instance the color specified by the experimenter. The fifth subject invariably saw only a white sheet of paper. Extensive

hypnotic suggestion did not alter this behavior. Nevertheless, the entire experiment was performed on this subject.

3. The 4 subjects who hallucinated the specified color invariably declared the succeeding sheet to be of the appropriate complementary color. There were no failures, except those of the fifth subject.

4. The naming of the control pairs of colored sheets was done correctly by all five subjects.

5. The repetition of the experimental procedure yielded results identical with those first obtained.

6. The questioning of the subjects concerning the color intensity of the various sheets exhibited to them elicited the information that the sheets described by the experimenter were brightly hued but that the sheets described by them were 'softer,' 'duller' and 'nowhere near so bright.' For example, to quote verbatim from one of the subjects, "You showed me a blue sheet and then later I saw a blue sheet. They were both blue in color but it wasn't the same blue. Mine was a lot duller, easier to look at and not so bright, while yours was very bright."

7. The word association test given immediately after the experimental procedure and with the subjects in the trance state yielded a total of 9 instances of color associations. These were: red-brown; red-pink; yellow-orange; yellow-tan; yellow-green; blue-red; blue-purplish; *yellow-blue* and *red-green.* One of the two associations of complementary colors was given by the fifth subject.

8. The word association test given after awakening the subjects yielded only two color associations, neither of which was of complementary colors.

9. The post-experimental questioning of the subjects disclosed them to be unable to define or to name complementary colors when asked directly. When a definition had been given them, and they were asked to hazard guesses, their tendency was to contrast light and dark colors, such as pink and brown.

Summary of Results

These experimental findings may be summarized briefly as follows: Of the 5 hypnotized subjects employed, 1 failed completely to meet the test situation. The remaining 4, in response to appropriate hypnotic suggestions, hallucinated as red, blue, green, or yellow the alternate sheets of white paper exhibited to them and invariably described the intervening white sheets as of the complementary color appropriate to the hallucinated color of the preceding sheet. The results obtained from the control procedures indicate clearly that the experimental findings derived directly from the procedure employed.

Comment

In reporting this experiment, the primary purpose is simply to present factual findings. Whatever the significance of the findings may be, their general limitations and their lack of scope, coupled with the present inadequate understanding of color vision, preclude any attempt at a theoretical elaboration in relation to color vision, and limit discussion to tentative suggestion of the processes by which the results may have been achieved.

An immediate consideration for discussion is that of the role of the hypnosis employed. Although the hypnotic state constituted an integral part of the procedure, its role may safely be described as only one of providing a setting favorable to the performance of the experiment. Unquestionably, the experiment could be repeated in the waking state, but probably with less ease and effectiveness. Hence, in any analysis of the results the question of hypnosis as such may properly be disregarded.

The next question concerns the character of the stimuli given to the subject. Obviously, the retinal stimulation by white paper did not enter directly into the situation except as a part of the experimental setting, although indirectly it may have had definite, though remote, significance. As for color stimuli, these were wholly auditory in character and were so given as to derive any significance entirely from the subject's experiential past. Hence, a definite statement

may be made that the responses elicited arose primarily from activity of central processes, and that the entire problem thus becomes essentially one of central, rather than peripheral, processes and activities.

For convenience of discussion, two possible interpretations of the findings obtained may be suggested, although, in all probability, they represent but partial aspects of a single possible explanation.

The first of these is the interpretation of the findings as the result of purely psychological or associational processes. To explain briefly, the experiencing of a color is ordinarily succeeded, though seldom consciously realized, by a negative after-image. In consequence of this sequence of psychophysiological processes there is established a direct association between their experiential aspects. Hence, stimuli serving to arouse directly the experiential values of a color would presumably serve to a somewhat lesser extent to arouse the experiential values of the intimately associated negative after-image.

To illustrate directly from the experiment, auditory stimuli, limited in character to the subject's concept of a specific color, were given. These stimuli served to arouse in the subject the associational activities concerned in actually experiencing the specified color. This situation was immediately succeeded by one favorable to the subject's becoming aware of any sequential associated experiential activity, namely, that deriving from the negative after-image or the complementary color.

Since no actual color stimuli were given to arouse neurophysiological activity on which could be based the initiation of experiential processes and activity, and since each of the 4 subjects who entered into the experimental situation showed no failures for any of the 16 test instances, the assumption may be warranted that there is a direct associational bond between the experiential values of a color and its complement sufficiently strong to permit its evocation by purely psychological stimuli.

While this explanation does constitute a possible inter-

pretation of the experimental findings and is applicable in many regards, it is unquestionably inadequate and far too simplified. Hence, a second interpretation, based upon psychophysiological processes and actually inclusive of the foregoing explanation as well as of other factors, may be suggested. However, the inadequacies of our present understanding of color vision preclude more than a statement of these possible factors.

First to be mentioned is the effect of the instruction to *see*. This instruction in itself, even if the subject had his eyes closed, would constitute an actual stimulus serving to arouse into activity various psycho-physiological processes preliminary to vision and upon which visual activity could be based. But since the subjects actually did receive visual stimulation, though neutral in character so far as color vision was concerned, there was some actual visual activity, with a consequent augmentation of the psycho-physiological processes involved in vision previously aroused by the nature of the situation.

Also, the suggestion to see a specific color, in itself, would serve to establish a certain 'mental set,' leading to the various preliminary psycho-physiological processes upon which could be based the initiation of the actual activity of color vision, and which would be derived from the psycho-physiological activities based upon past learning. Thus, the specifying of a definite color, while constituting a purely psychological stimulus, would stimulate various mental processes intimately related to parts or aspects of the psychophysiological activity of vision already in progress, and these, in turn, would serve to reinforce each other.

In addition, the naming of a specific color would stimulate into activity various mental processes related to the experiential values of the chosen color and these processes would, in turn, serve to augment the activity of the other processes already aroused.

Hence, although no visual color stimulus had been given, there would be active various psycho-physiological and experiential processes, all of which would be intimately

associated with each other and ordinarily contingent upon color stimulation of a particular character.

Furthermore, the character of the suggestions to see the specified color was such as to create a state of tension in the subject compelling a response but limiting the possibility of a response strictly to the subject's own concept of the color. Thus, his response could derive only from his own experiential associations and learned activities. From this state of tension with its limitations, there could derive, presumably, a process of control and direction of the various psycho-physiological processes in activity leading to the color vision response normally contingent upon such processes.

Thus, taking the place of an actual color stimulus, there would be the interaction of these various processes, each serving to initiate further and more complete activity comparable to that of the actual processes of color vision, with the entire process culminating in an actual psychological response of color, the validity of which was clearly indicated by the ensuing subjective response of the complementary color.

When the subjects had made their response to the various psycho-physiological processes aroused in them by the presentation of the stimulus sheet described as colored by the experimenter, there followed the withdrawal of this sheet and the presentation of a second sheet which they were to describe as colored. This withdrawal and replacement of sheets constituted an actual stimulus for the cessation of the psycho-physiological activity in progress, thereby initiating directly the re-adjustive activities consequent upon the cessation of psycho- and neuro-physiological activity. Since the subjects invariably described the second sheet of each pair as of the correct complementary color, the assumption is warranted that re-adjustive processes and activities did occur and that these corresponded in character and degree to those consequent upon actual color vision, and were sufficiently diverse and strong as to permit a subjective response comparable to that deriving from a negative after-image.

Conclusion

To conclude, emphasis must be placed entirely upon the factual findings rather than upon the attempted interpretations. The essential consideration of the experiment is that the hallucination of various colors was sufficiently valid subjectively to permit a spontaneous and invariable hallucination of complementary colors somewhat in the form of negative after-images.

Unquestionably, associational and neuro- and psychophysiological processes entered into the production of the phenomena, but not necessarily as has been suggested. Further development of the understanding of color vision is requisite for any adequate interpretation of the experimental findings, and hence, their significance must be restricted to their evidential values in relation to the importance of cortical processes in color vision.

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A CASE OF EXPERIMENTALLY INDUCED NEUROSIS IN THE CAT

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In a recent study concerning the behavior of cats on the double alternation problem in the temporal maze Karn (**3**) obtained positive results. As Hunter (**2**) has indicated, solution of this problem calls for response to temporal relations in the absence of differential sensory cues and therefore appears to depend upon an implicit symbolic process. Following mastery of the double alternation response one animal, a cat six months old, ceased correct performance of the response and subsequently exhibited symptoms characteristic of a case of experimentally induced neurosis. The present report is concerned with a description of the behavior of this animal. By means of the conditioned reflex technique similar behavior has been obtained in the dog (Pavlov, **5**) and in the sheep (Liddell, **4**). Such studies may disclose important information bearing upon the etiology of mental disorder in human beings.

A ground plan of the apparatus used in the above-mentioned study of double alternation in the cat is shown in Fig. 1. The problem confronting the subject after it has entered the maze at *E* is to proceed up the central alley to *x*, make a turn to the right and continue around the right side of the maze to the front. This constitutes the first response of the trial. Following feeding at F_2 the subject is permitted to enter the central alley and should this time repeat his previous performance, thus completing the second response. The third and fourth responses, which complete the trial, should be made around the left side of the maze. There are thus four choices to be made at *x* conforming to the order right, right, left, left. During the training procedure doors 1, 2, 3, and 4 are used to prevent completion of an incorrect response and to prevent retracing. In control experiments all doors except *E* remain open throughout an entire trial. For details of the training procedure the reader is referred to the original account (**3**).

The subject under discussion learned the double alternation response in 230 trials, the average accuracy of response on trials 201–230 being 90 percent (108 responses correct out of

120). Up to this time this cat always entered the maze readily, worked rapidly and was, in general, a docile subject. In order to determine whether he could achieve still higher accuracy, training was continued beyond the 230th trial.

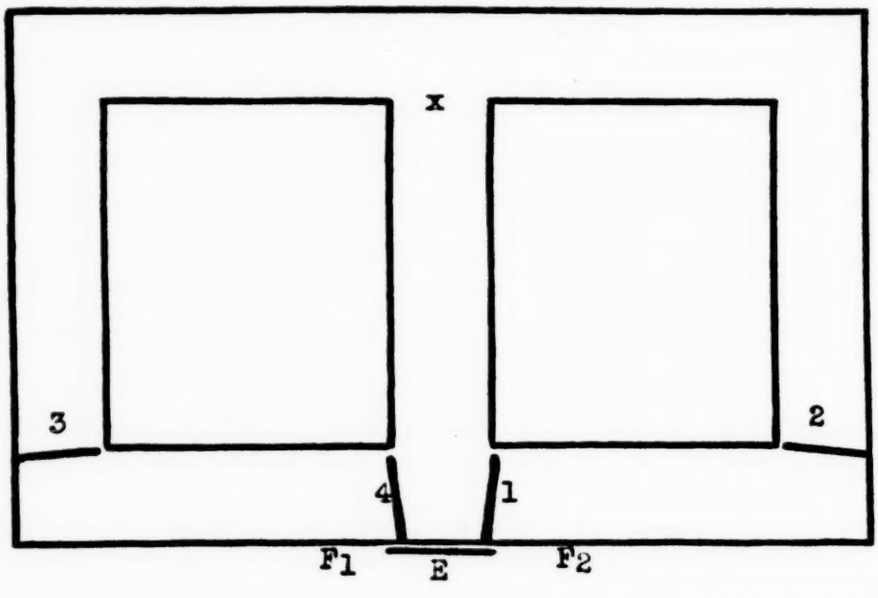

FIG. 1. Ground plan of temporal maze used in the study of the double alternation response.

On the second response of the 232nd trial a radical change in behavior was observed. The animal hesitated much longer than usual at the choice point, finally jumped into the right back alley and then raced to the front of the maze. During the remainder of the trial he worked slowly and whimpered continually. Five hours later when brought to the maze for another trial he refused to enter of his own accord and resisted being forced through the entrance door by scratching and clawing at the experimenter and apparatus. After the cat had been forced into the maze, its behavior throughout the trial was characterized by refusal to work at first, scratching at the doors and wire mesh which covered the apparatus, and loud meowing and urination at the choice points. Such disturbance, which had never appeared prior to the 232nd trial, was always most pronounced just preceding the second response when the turn into the right back alley was to be made. This response was by far the most difficult of the

right, right, left, left sequence for the animal to master during the training and was apparently taxing his discriminative ability to the limit. On subsequent trials the animal's actions were similar to those just described except that the 'emotional' disturbance appeared to get worse. Occasionally on the second response of a trial he made a correct choice by entering the right back alley. However, after proceeding a few steps, he turned quickly and raced back into the central alley or around the left side of the maze. As a result of this breakdown the subject's accuracy of performance became progressively poorer. Thus, on trials 231–240 only 25 responses out of 40 were correct (accuracy 62.5 percent); on trials 241–250, 22 out of 40 were correct (accuracy 55 percent); on trials 251–260, 23 out of 40 were correct (accuracy 57.5 percent); and on the last four trials of the experiment, nine responses out of 16 were correct.

It is significant to note that during the 32 trials given the animal after the onset of the above-mentioned disturbance, he responded right, left, left, right 23 times. Eighteen of these sequences occurred during the last 24 trials. This manner of responding had appeared during the early stages of training and had persisted for about 75 trials, finally giving way to the double alternation response (right, right, left, left). Only twice during the last 32 trials did the animal respond right, right, left, left. This reversion to a lower-order habit is apparently a clear case of regression as defined by Hamilton and Krechevsky (1). It is, to use their terms, ". . . a reversion to an earlier, well-established mode of behavior, and persistence in that mode despite the relative inefficiency of that behavior in solving the problem confronting the organism." They assume that regression arises when the individual is placed in a strong emotional situation. These observations indicate that, in the presence of a difficult problem, the normal relations between excitation and inhibition were so upset that the cat could no longer perform double alternation. Furthermore, the animal apparently attempted to resolve the conflict by regressing to a lower-order habit, persisting in this despite the fact that optimal conditions of

adjustment did not result. Such an outgrowth of behavior crises has been called a neurosis.

(Manuscript received February 5, 1938)

References

1. Hamilton, J. A., and Krechevsky, I., Studies in the effect of shock upon behavior plasticity in the rat, *J. Comp. Psychol.*, 1933, **16**, 237–253.
2. Hunter, W. S., The behavior of raccoons in a double alternation temporal maze, *J. Genet. Psychol.*, 1928, **35**, 374–388.
3. Karn, H. W., The behavior of cats on the double alternation problem in the temporal maze, *J. Comp. Psychol.*, 1938 (in press).
4. Liddell, H. S., The conditioned reflex, in *Comparative Psychology*, F. A. Moss (Ed), New York: Prentice-Hall, 1934.
5. Pavlov, I. P., *Lectures on Conditioned Reflexes*, Internat. Pub., 1928.

SUSCEPTIBILITY TO OPTICAL ILLUSIONS: SPECIFIC OR GENERAL?

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Susceptibility to the more common geometrical optical illusions is practically universal. In any series of measurements, however, it is customary to find wide individual differences in degree of suceptibility to any particular illusion. With the Müller-Lyer figure, for example, the error of judgment in adjusting the lines to apparent equivalence ranged from 3 to 35 mm in a group of 100 subjects when the standard line was 80 mm long. We are not concerned here about the cause of the differences. The question is whether the person who makes a large error in adjusting the Müller-Lyer figure also has a large error of perception in viewing the vertical-horizontal or the Poggendorff figure, and whether the tendency to commit a small error of judgment in one corresponds to a similar tendency with the other figures. In other words, is susceptibility to geometric illusions a factor common to all the figures or is it specific to each illusion situation? Thus, the purpose of this study is to investigate the degree to which susceptibility to error of judgment in viewing the Müller-Lyer, the vertical-horizontal and the Poggendorff illusions is a specific trait.

The apparatus was constructed from cardboard. Each illusion figure was drawn in black ink on gray cardboard, 7 by 12 inches, surrounded by a black frame. One part of the figure was fixed and the other adjustable. In the Müller-Lyer figure the fixed line was 8 cm long and was inclosed by oblique lines, 40 degrees to the horizontal, and the segment in which the oblique lines made obtuse angles (140 degrees) was adjustable. In the vertical-horizontal figure the horizontal line, 8 cm long, was stationary and the vertical line adjustable. The two parallel lines in the Poggendorff were 14 cm long and 2 cm apart. The oblique, 30 degrees to the parallel lines, was fixed at the left side and movable at the right. On the back of the sliding part of each figure was a millimeter scale so arranged that the amount of error in the subject's adjustment of the figure was easily read off for recording.

The subject sat comfortably at a table with the illusion board in his hands, about 15 inches from his eyes. The line of vision was perpendicular to the figure. With the

Müller-Lyer and the vertical-horizontal figures the subject was asked to move the adjustable portion of the figure until the two lines appeared equal, and in the Poggendorff figure, until one oblique appeared to be a continuation of the other. He was urged to make the judgment carefully but promptly. All subjects who had been exposed to psychology were asked to judge entirely in terms of apparent equality or apparent continuation of the lines.

There were eight adjustments made for each figure. In the Müller-Lyer figure there were four trials with the adjustable portion to the right and four with it at the left. The adjustable part was longer and shorter than the fixed portion on alternate trials. With the vertical-horizontal figure, there were four trials with the adjustable portion longer than the horizontal and four with it shorter, alternating from trial to trial. In the Poggendorff figure the trials were begun with the adjustable oblique alternating higher and lower than the fixed oblique.

Three separate groups were measured.[1] In the first group there were 64 University students, including freshmen, sophomores, juniors, seniors, and graduate students; in the second, 100 adults ranging from 17 to 40 years of age; in the third, 35 children ranging from 9 years to 9 years and 11 months of age. There were males and females in each group.

TABLE 1

MEAN ERRORS IN CENTIMETERS WITH S.D.'S

Illusion	Mean Error	σ
Experiment I: N = 64 University students		
Müller-Lyer	−2.39	.66
Vertical-horizontal	−1.39	.53
Poggendorff	.65	.41
Experiment II: N = 100 subjects, 17 to 40 years		
Müller-Lyer	−1.59	.37
Vertical-horizontal	−1.06	.30
Poggendorff	.66	.24
Experiment III: 35 children, 9 to 10 years		
Müller-Lyer	−2.16	.70
Vertical-horizontal	−0.44	.73
Poggendorff	1.46	.46

In computing the central tendency for an individual's 8 scores, the mean error was employed, taking into consideration both plus and minus scores. As a matter of fact there were few reversal scores. In Experiment II (800 measurements for each illusion) there were 33 reversal scores out of 800 in the Poggendorff; 1 out of 800 in the Müller-Lyer; and 55 out of 800 in the vertical-horizontal illusion. Constant errors were also computed in this experiment (II). The trend of results varies little from computations employing the mean errors.

[1] Thanks are due to Saadia Gelb, Albert G. Buelow and Virginia Wallis who helped to collect the data in these experiments.

The mean errors and the corresponding standard deviations for the three experiments are listed in Table 1. The trend of the mean errors are quite similar in Experiment I and II where adult subjects were employed. The Müller-Lyer has the largest error, the vertical-horizontal comes next, and the Poggendorf has the smallest error. The smaller standard deviations in Experiment II are undoubtedly due to the larger number of subjects in this experiment. In the experiment with children the Müller-Lyer illusion also yielded the largest mean error. The relative size of the error is reversed, however, for the other two illusions. This may be a function of either the age of the subjects or the size of the sample.

TABLE 2

RELIABILITY OF MEASUREMENTS

(The coefficients were computed by the split-half method and corrected by the Brown-Spearman prophecy formula)

Illusion	r	$B - Sr_x$
Experiment I: N = 64 University students		
Müller-Lyer	.94	.97
Vertical-horizontal	.82	.90
Poggendorff	.70	.82
Experiment II: N = 100 subjects, 17 to 40 years		
Müller-Lyer	.92	.96
Vertical-horizontal	.92	.96
Poggendorff	.91	.95
Experiment III: N = 35 children, 9 to 10 years		
Müller-Lyer	.92	.96
Vertical-horizontal	.95	.97
Poggendorff	.80	.89

The reliabilities of the scores are shown in Table 2. The reliability coefficients were computed by the split-half method (trials 1 + 3 + 5 + 7 vs. trials 2 + 4 + 6 + 8) and corrected by the Brown-Spearman prophecy formula to predict the reliability of the complete series of measurements. The corrected coefficients range from.82 to .97 in Experiment I, from .95 to .96 in II, and from .89 to .96 in III. In every

instance the measurements of the Poggendorff illusion yielded slightly lower coefficients than the other two illusions. All reliabilities, however, are high. These results confirm other reports that a reliable score can be obtained by averaging 8 trials in this kind of measurement.

The degree to which susceptibility to one illusion predicts degree of susceptibility to the other two illusions is shown by the intercorrelations in Table 3. The results are fairly con-

TABLE 3

INTERCORRELATIONS FOR EXPERIMENTS I, II AND III *

	I	II	III
Müller-Lyer vs. vertical-horizontal	.12	.36	.33
Müller-Lyer vs. Poggendorff	.03	−.11	.13
Vertical-horizontal vs. Poggendorff	.08	.00	.04

* When the constant error instead of the mean error was employed with data in Experiment II, the correlations, listed in the same order as in Table 3, were respectively .50, .22, and .04.

sistent from one group of subjects to another whether the mean error or the constant error is employed in computing averages. It is clear that amount of error in adjusting the Poggendorff figure gives no indication of the error to expect in either the Müller-Lyer or the vertical-horizontal illusion. The average intercorrleation for these comparisons is .03 (mean error scores). The degree of the Poggendorff illusion, therefore, is specific with respect to the other two illusions.

Apparently susceptibility to the Müller-Lyer illusion and to the vertical-horizontal illusion are related. While the correlation in Experiment I is not significant it is in the same direction as the correlations in Experiments II and III, which are fairly stable. These coefficients of .36 and .33 indicate a slight positive relationship. This tendency is more pronounced when, in Experiment II, the constant error method of averaging is used. Here the correlation between the Müller-Lyer and the vertical-horizontal illusion is .50. It may be inferred that susceptibility to the Müller-Lyer illusion predicts to a moderate degree the susceptibility to the vertical-horizontal illusion. That is, these two illusions are

not entirely specific with relation to each other. This is possibly due to the fact that both involve judgments of visual extent.

Summary

1. The amount of error in viewing the Müller-Lyer, the vertical-horizontal, and the Poggendorff illusions was measured.

2. Three groups of subjects were employed: (*a*) 64 University students, (*b*) 100 adults, and (*c*) 35 children, 9 to 10 years old.

3. The results were fairly consistent from group to group whether the average amount of error is expressed as the mean error or as the constant error.

4. Susceptibility to the Müller-Lyer illusion is correlated to a moderate degree with the vertical-horizontal illusion. Error in the Poggendorff illusion, however, is not related to the amount of error in either of the other two illusions.

5. The data justify the conclusion that, considering these three illusions only, susceptibility to the Poggendorff illusion is specific, and that susceptibility to the other two illusions has some element in common.

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A DIRECT READING CARDIO-CHRONOSCOPE *

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Investigations of cardiac changes in emotional situations, and during exercise and recovery, are greatly facilitated by the use of an instrument which indicates the pulse rate directly and continuously. Fleisch (**3**) has developed such an apparatus, but for various reasons it has failed to come into general use.

It has been found possible to use an electrical method for directly determining the heart rate, through the use of a principle outlined by Maxwell (**5**) and employed by Case (**2**) for measuring the frequency of electric currents as low as 5 cycles per second. Because the heart rate in man is much slower than this, it was necessary to modify the apparatus considerably.

Experimental investigation of several methods led to the development of the apparatus shown in Fig. 1. It will be seen that the capacitor C_1, is charged by a constant potential of 60 volts, and at each heart beat the electric charge is transferred through the action of the relay armature to another capacitor C_2 which is relatively large. The charge on C_2 is leaked off by the resistors R_5 and R_7 (R_3 can be neglected, since it is small). The potential across R_5 is measured with a thermionic voltmeter V_1. The time-constant of the combination C_2-R_7-R_5 is sufficient to filter the discharge frequency so that its average value can be determined down to about 2 per second. For lower pulse rates the filtering is inadequate, and cannot be improved by increasing the time constant without making unreasonably long the period over which the integrating action of the apparatus extends.

The difficulty is overcome by adding another thermionic tube V_2, the grid of which is coupled to the grid of the first

* From the Research Laboratory, Department of Physical Education for Men.

voltmeter tube through the capacitor C_3. The cathode-anode circuits of the two tubes, together with resistors R_9 and R_{10}, form a Wheatstone bridge. Since the surge component of the voltage across R_5 can pass through C_3, it effects the grids of

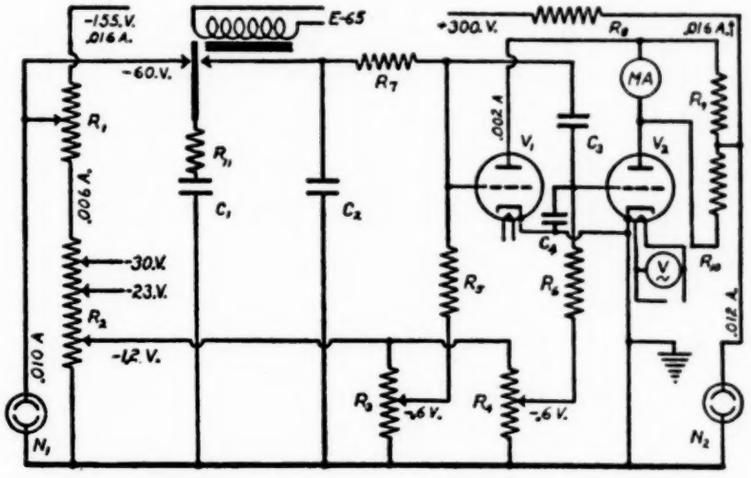

FIG. 1. The cardio-chronoscope.

C_1 = .04 mfd. (Mica).
C_2 = 4 to 12 mfd.
C_3 = 4 mfd.
C_4 = .5 mfd.
R_1 = 10,000 ohms.
R_2 = 5,000 ohms.
R_3, R_4 = 1,000 ohms.
R_5, R_6 = ¼ megohm.
R_7 = ½ megohm.
R_8 = 15,000 ohms.
R_9, R_{10} = 10,000 ohms.
R_{11} = 5,000 ohms.
N_1, N_2 = 2 watt G.E. neon, base resistor short-circuited.
(All resistors are wire-wound)

both tubes and cancels out. The steady part of the potential reaches the grid of only the first tube (since it is blocked from the grid of V_2 by C_3), and results in a deflection of the meter MA that is proportional to the pulse rate. Capacitor C_4 is necessary to maintain the correct phase relationships.

It is of course necessary to operate the tubes on the proper part of their grid-plate curves. With the relay inoperative, R_3 is adjusted so that the meter MA reads zero, then R_3 and R_4 are simultaneously changed (keeping MA at zero) until an auxillary meter in the plate circuit of V_1, shows a current flow of 2 milliamperes. R_3 is now locked in position, and R_4 used to compensate for any shift in the operating zero of MA. Naturally the zero adjustment should only be made when the tubes are thoroughly warmed up. The vacuum tubes used in the apparatus in its present form are type 6C5.

It is quite important that they be operated at their full rated heater voltage of 6.3.

The operating coil of the relay (Western Electric E-65) is connected in series with the plate of the gas-triode tube used in a cardiac potential amplifier described in an earlier note (4). Referring now to the diagram of that apparatus, it is necessary to short-circuit and remove the 10,000 ohm resistor that was between the plate of the 5Z4 tube and the filament of the 2Z2. The 2 mfd. capacitor connected to the plate of the 2Z2 is replaced with one of about 0.3 mfd., the exact value being experimentally chosen to produce the proper rectifier voltage. The output lead of the 250 H. choke fed by this tube is disconnected from the 4,000 ohm resistor (which is no longer used) to become the − 155 V. lead of the present circuit, and the point marked 300 V. is connected to the 300 volt lead. It has also been found advisable to use only a single bias cell for the 6J7 tube, and to place it directly in the grid lead instead of in the grid return as shown in the amplifier diagram. The 20,000 ohm resistor supplying the plate of the gas triode needs to be reduced somewhat to compensate for the voltage drop across the relay coil. Additional work with the amplifier has shown that when it is used without the filters, as is the case in ordinary nerve-potential measurements, the 6F6 tube should be operated under self-bias conditions.

The meter *MA* is of the miniature type having a $3\frac{1}{4}$ inch scale and a rather heavy pointer. For hospital and operating room use, or for class-room demonstration, this may be replaced with a larger meter. A recording meter could be used. Adjustable maximum and minimum contacts (not shown in the diagram) operate thermonic relays which signal when the pulse rate exceeds or drops below pre-determined limits. The neon bulbs N_1 and N_2 are effective in stabilizing the D.C. voltages. A variation of 15 percent in supply line voltage results in a change on the meter dial of only about 3 percent. The error is apparently due to alteration of cathode temperature. Manual voltage regulation by means of a resistor in series with the power transformer should be used

if supply line conditions are uncertain or if highest accuracy is desired.

The length of the integration period may be altered by changing the size of C_2, although it must be kept in mind that any change such as this alters the calibration curve. When C_2 is made smaller, R_6 must be increased to supress the individual pulse beats. Using the circuit conditions shown in Fig. 1, the relation between meter deflection and pulse rate is substantially linear up to the full scale deflection of 200 beats per minute, and the integration time is of the order of 6 seconds. This linearity of scale is an advantage as compared with the geometric scale that is characteristic of mechanical rate-recorders such as the *Zeitschreiber* (**3**). The pulse rate at which full scale deflection occurs may be changed by altering the size of C_1.

Another circuit should be briefly mentioned, which does electrically (and more simply) just what the *Zeitschreiber* does mechanically. A capacitor C is continuously connected through a high resistance to the charging voltage. With each heart beat, the relay momentarily short-circuits and discharges the capacitor. If the time-constant of the circuit is suitably chosen, the peak voltage across the capacitor is inversely proportional to the pulse frequency. A thermonic voltmeter using a short-period galvanometer could be used to measure this voltage; the preliminary tests have employed a cathode ray tube. Obviously, photorecording is necessary with this method.

(Manuscript received January 15, 1938)

References

1. Boas, E. P. and Goldschmidt, E. F., *The Heart Rate*, Thomas, Baltimore, 1932.
2. Case, N. P., A precise and rapid method of measuring frequencies from 5 to 200 cycles per second, *U. S. Bur. of Stds. J. of Res.*, 1930, **5**, 237–242.
3. Fleish, A., Der Pulszeitschreiber; ein Apparat zur Aufzeichnung der zeitlichen Pulsindervalle als Ordinate, *Ztschr. f. d. ges. exper. Med.*, 1930, **72**, 384–400.
4. Henry, F., An electric cardiometer, *Science*, 1937, **86**, 229–230.
5. Maxwell, J. C., *Electricity and Magnetism*, Vol. II, Clarendon Press, Oxford, 1892.

'EXTRA-SENSORY PERCEPTIONS' OR INSTRUCTIONS?

BY SAMUEL W. FERNBERGER

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During the recent months there has been a great deal of publicity regarding experiments which were alleged to deal with what has been called Extra-Sensory Perception or the ability of an individual to have knowledge of some object by means other than the ordinarily recognized sensory channels. This new version of the old problem of mental telepathy has received much notice since the publication of Rhine's book.[1] Some of this publicity has been confined to scientific journals but much has appeared in the public press and one commercial firm (The Zenith Radio Corporation) has used this method as the basis for a weekly radio program.

There are some psychologists who regret the popular publicity both by radio and by the public press. These psychologists believe that it is too early to popularly publicize these experiments before they have been adequately tested and accepted by the scientific world. They also regret that this popular publicity has ignored a number of experimental reports which have completely failed to confirm the results of Rhine and some of the other workers who have obtained favorable results.

Beginning in September, 1937, the Zenith Radio Corporation has attempted to test the validity of Extra-Sensory Perception (hereafter designated as ESP) as part of their weekly radio program. The statistical results of the first 8 weeks of tests were made available to psychologists without interpretation and without comment. The following test method was employed: two contrasting objects (black-white; circle-cross, star-wavy line, etc.) were chosen for a particular

[1] J. B. Rhine, *New Frontiers of the Mind.* N. Y.: Farrar and Rinehart. 1937, Pp. 275.

week and the choice of objects announced to the listeners over the radio. A roulette wheel was prepared with a equal number of holes designated for these objects. The ball was spun in the roulette wheel and the first choice made in this manner. The radio audience were asked to write their first choice on a prepared slip and the ball was spun a second time. Five or seven choices were made each evening. The slips with the judgments were then sent to the studio where the results were tabulated and the results and choices not announced until the following week. Presumably these were tests of the ability of an individual to know by some means other than the normal sensory channels what was happening many miles or hundreds or thousands of miles away. It is interesting to note that the largest number of replies recorded for any one week was 17,239.

Under date of February 1, 1938, Mr. E. F. McDonald, Jr., President of the Zenith Radio Corporation, asked the cooperation of various departments of psychology to participate in a test in which the situation was to be reversed. Each department of psychology was to select 10 individuals who, at the time of the broadcast, were to be isolated in a room where they could not possibly hear a radio. This room was connected with a radio room by an electric signal bell. The selection was again to be made by the wheel. It was to be announced over the radio and the listeners 'from coast to coast' were instructed to think of that selection for 5 seconds. Then a signal was given over the radio. Immediately the one observer in the radio room rang the signal bell in the isolated room where there were 10 subjects and each of these were asked to write their first choice on a prepared pad. This was repeated five times. Presumably this procedure was to test the effect of many thousands of minds in the isolated observers.

Black and white were chosen as the two contrasting objects for this week. In the absence of more specific instructions for the observers, the author (who was in the radio room) read the following quotation from the "Directions for the Test" to the 10 individuals from the University of Pennsylvania who participated in this test as observers.

We would like to request your coöperation in this important test by having ten of your people who are interested in the subject in a closed room during this half hour period *with no radio set in the room and no means of knowing through radio whether black or white has been selected.* The room in which these ten interested people are located with pad and pencil before them should be connected by an electric bell or some other means of signalling with the room in which the radio set is located so that whoever is in charge of the radio set can signal to the receivers by bell or other signal when the selecting machine has selected either black or white allowing the five second period of concentration for each selection. In other words the announcer will say to the public "The selecting machine has now selected black. Will you concentrate for five seconds on black." When the announcer says this a signal should be given by your radio operator to your listeners indicating that the five second period of concentration has started and another signal indicating the end of the five second period. This will be repeated five times as the various selections are made.

This extract from the 'Directions' was read to the listeners in the absence of more specific instructions for them and one must suspect that this or a similar procedure must have been followed elsewhere by other coöperating departments. The test was held on the evening of February 13, 1938 and, on that evening, it turned out that all five selections were black. The probability for this occurrence is not great, being only one in thirty-two.

On February 14th the results were sent to Mr. McDonald and, at that time, the writer indicated in a covering letter that he believed, as a test of ESP, it was most unfortunate that the drawings had all come black for two reasons: 1, that people think in the black-white sequence rather than in the reverse order (one "puts it down in black and white; the color of a setter dog is black and white and there is even a brand of whiskey with that name) and 2, because the instructions to the listeners were 'loaded' for black, *i.e.* 'black-white' was used twice in that order and the example was in terms of a black choice.

Under date of February 17, 1938, a letter was received from Mr. John G. Brooks in the absence from the city of Mr. McDonald. This letter indicates that it is his belief that people are more apt to think in the white-black order and he bases his opinion on the results of previous black-white tests made over the radio. These specific results are not known to the present writer.

It was thus decided to put the matter to an experimental test. This test was made during the week of February 21, 1938, on 700 students in the various classes of psychology at the University of Pennsylvania. Group tests were made throughout—the number in any group never exceeding 60 students. A paper was given to each individual and the following instructions were read to them by the experimenter.

I am going to think of a color and I want you to write down the color of which you believe I am thinking. The colors will be either black or white. I shall tap on the table when I begin to think of the first color and, five seconds later, I shall tap again on the table. If you think that I have been thinking of black during this five second interval, you will write black in the first space. This will be repeated five times as the various selections are made.

It is obvious that the Zenith instructions had to be modified to meet this new experimental situation. There was as little modification made as possible. But these present instructions were 'loaded' for black as we believe the previous ones had been and so, in these instructions, the black-white sequence was used once and the example was in terms of a black choice.

There was one other essential difference in the two experimental situations. In the Zenith set-up the some thousands of the radio audience concentrated on the announced choice. In the Pennsylvania situation no choice was made and the experimenter carefully refrained from thinking of either black or white. To insure this during the five second interval between the two signals, he set himself such tasks as naming the Presidents of the United States in order.

The results of the Zenith Radio Corporation tests were received on March 21, 1938, with a covering letter dated March 16th. The present paper is concerned with only one phase of the situation, namely, the occurrence of 'black' judgments on each of the five successive choices. These results are given in Table I for the 339 individuals whose returns were tabulated in the Zenith experiment and for the 700 individuals who acted as observers in the Pennsylvania experiment. In the first two columns are the actual number of black judgments for each choice and in the last two columns these numbers are reduced to percentages for greater ease of comparison.

TABLE I

FREQUENCY OF JUDGMENTS IN AN ESP SITUATION

Choice	Number of Black Choices		Percentage of Black Choices	
	Zenith 13 Feb. 1938 N-339	Pennsylvania Feb. 1938 N-700	Zenith 13 Feb. 1938 N-339	Pennsylvania Feb. 1938 N-700
1	191	423	56.3	60.4
2	165	347	48.7	49.6
3	161	317	47.5	45.3
4	179	350	52.8	50.0
5	157	354	46.3	50.6

It will be observed that the highest percentage of black judgments in both sets are for the first choice. And also in both cases the next highest percentage of black judgments occurred for the fourth or fifth choice. The percentages for the second and third choices are lower than either of these others in both experiments.

These findings become even more impressive when one fractionates the data for the Pennsylvania experiments in the order in which these results were obtained. These data will be found in Table II. In 5 of the 6 fractions, the first choice

TABLE II

FRACTIONATION OF PENNSYLVANIA RESULTS

Choice	Fraction 1 N-125	Fraction 2 N-125	Fraction 3 N-125	Fraction 4 N-125	Fraction 5 N-125	Fraction 6 N-75
1	85	67	70	75	77	49
2	60	59	70	61	62	35
3	47	58	58	52	60	42
4	63	71	53	63	71	29
5	61	57	68	65	58	45

shows the highest percentage of black reports and in this case (Fraction 2), the first choice shows the second highest percentage. In regard to the fourth choices, the results indicate in one case the highest percentage (Fraction 2); in two cases the second highest percentage (Fractions 1 and 5) and in one case the third highest percentage (Fraction 4).

These results can be explained in terms of two well known psychological principles. The high percentages for black

found in the first choices is the direct result of mental set and is merely a reflection of the 'loading' of the instructions and of the more general mental set to think in the black-white sequence rather than the reverse. The lower percentage values for the second and third choices is to be explained in terms of the well-known psychological principle of the tendency to avoid repetition of a particular judgment in any series of judgments. This effect of avoidance of repetition of judgment has been clearly demonstrated for psychophysical judgments by Preston [2] who shows that such avoidance may have a demonstrable effect even after the first succeeding judgment. The tendency for the increased percentages of black reports for the fourth choice may be explained by the fact that this avoidance tendency has worn off by this time.

It is the desire of the writer to express a conclusion as cautiously as possible. But at least this much may be said: These results demonstrate that two fundamental psychological principles, namely 1, the presence of mental set due to instructions and 2, the tendency to avoid the repetition of judgments does not seem to be effected if 1, some thousands of minds are attempting to effect the judgments of 339 minds (the Zenith situation) or if 2, no mind is attempting to effect the judgments of 700 minds (the Pennsylvania situation). In both cases, these fundamental psychological principles seem to be equally effective in determining the nature of the judgment.

[2] M. G. Preston, Contrast effects and the psychophysical judgments, *American Journal of Psychology*, 1936, **48**, 389–402, and The effect of interpolated time intervals upon the contrast effects, *Journal of Experimental Psychology*, 1936, **19**, 706–715.

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Quarterly. Abnormal and social. Founded 1906.

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Quarterly. Psychoanalysis. Founded 1913.

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Monthly. Experimental psychology. Founded 1916.

Journal of Applied Psychology—Indianapolis; C. E. Pauley & Co.
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